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THE QUARTERLY JOURNAL OF EXPERIMENTAL PSYCHOLOGY

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THE QUARTERLY JOURNAL OF
EXPERIMENTAL PSYCHOLOGY

Vol. XI 1959 Part 1

EDITORIAL

The *Quarterly Journal of Experimental Psychology* was founded by the Experimental Psychology Group in 1948. Its avowed object was to foster an experimental approach to psychological problems and to provide scope for the publication of original work in this field. The first volume was completed in 1949 under the capable Editorship of R. C. Oldfield. For the next eight years, the Editorship was undertaken by D. Russell Davis, who devoted extraordinary skill and care to the affairs of the *Journal*. He was particularly concerned to establish and maintain firm scientific standards and to place the business management of the *Journal* on a secure foundation. In both these respects he succeeded admirably. Whatever repute the *Quarterly Journal of Experimental Psychology* may now possess is almost wholly due to his devoted service.

Dr. Russell Davis retired from the Editorship at his own wish at the end of 1957 and has been succeeded by O. L. Zangwill. The remaining members of the Editorial Board kindly agreed to continue their service for a further year to assist the new Editor. Their assistance to the *Journal* over a long period has been highly valued and is most gratefully acknowledged.

The present volume appears under the aegis of a new and enlarged Editorial Board, reflecting the increase in both the size and range of interests of the sponsoring body. It coincides, too, with the metamorphosis of the Experimental Psychology Group into the *Experimental Psychology Society*. This is the outcome of the very considerable expansion of experimental psychology in Great Britain over the past ten years. Whereas it is hoped that the new society will preserve all that was best in the old E.P.G., it is felt that the time has now passed when the growing needs of experimental psychology could best be met by the activities of a small discussion group. The *Experimental Psychology Society* has been conceived as a larger and more formally constituted body, adapted to assume a wider range of responsibilities in the promotion of its objects. Although a continuation of the Experimental Psychology Group, it is in effect a new Society.

The policy of this *Journal* will remain as stated in the *Editorial* to the first volume (Feb., 1948). As before, its aim will be to provide facilities for the publication of appropriate work in experimental psychology and related fields. Whereas it is likely that reports of experimental inquiries will continue to find predominant representation in its pages, we would wish to remind contributors that it was not the intention of the founders that only papers reporting strictly experimental investigations will find a place. Experiment signifies only in relation to theory, and the *Journal* is open to receive papers of a theoretical or critical nature, with the sole provision that they should embody direct and systematic reference to the findings of observation. The *Journal* is likewise interested in empirical work in any branch of psychology that might provide a basis for more systematic experimental study.

The *Quarterly Journal of Experimental Psychology* is open to contributors whether or not they are members of the Experimental Psychology Society. Further, it was

by no means the intention of the founders to limit the scope of the *Journal* exclusively to work emanating from Departments of Psychology. Whereas the primary aim of the *Journal* has been, and will remain, the elucidation of psychological problems, it is clear that much work directed towards this end nowadays falls within the scope of disciplines other than that of experimental psychology. Among these are neurophysiology and neuroanatomy, comparative zoology and ethology, experimental psychiatry, and some aspects of applied physics, in particular communications and control engineering. In view of these considerations, papers submitted to this *Journal* will be considered from the standpoint of their relevance to elucidation of behaviour and without reference to the nature of the Department or Institution in which the work had its origin. Needless to say, the Editor will also be glad to receive and consider appropriate contributions from workers in other countries.

The members of the Editorial Board believe that this policy is faithful to the intentions of the founders and reflects the principles embodied in the constitution of the *Experimental Psychology Society*. These principles they will endeavour to maintain to the utmost in the future conduct of the *Journal*.

LEARNING A PRESSURE UNDER CONDITIONS OF IMMEDIATE AND DELAYED KNOWLEDGE OF RESULTS

BY

JOHN ANNETT

From the Oxford University Institute of Experimental Psychology

The results of a series of experiments on learning to reproduce a precise pressure are described and discussed in relation to a tentative hypothesis about the importance of timing of feedback signals. The evidence supports the contention that a careful analysis of the perceptual aspects of performance is necessary to account for the acquisition and retention of a precise motor response. Whilst the original hypothesis is sustained, other factors leading to the same result are clearly important. New evidence is produced which strongly suggests that cues presented simultaneously in two modalities interact. A possible distinction between sensory feedback which is used to control a response and feedback which cannot so be used, and the relation of this distinction to the learning of the response is also discussed.

Introduction

In two recent papers (Annett and Kay, 1956, 1957) it was suggested that a consistent account of the acquisition of skill could be built up from an application of the concepts of information theory to the receptor side of a motor performance. This emphasis on the organization of incoming data indicates the importance, for learning, the relationships between sets of relevant input data arising from a response. It is suggested that during the course of practice the "informativeness" of some cues may change such that erstwhile sources of information later become redundant. The mechanism implied is a straightforward prediction process. For a number of reasons, the new trainee needs more actual data from which to extract essential information and sometimes this extra data is supplied as knowledge of results. It is also common practice to incorporate warning signals and other special cues in training devices. Seymour, for example, has suggested that a set of kinaesthetic data may be presented simultaneously to the trainee in the form of a visual scale which will enable him to "identify" and learn the correct "feel" of a control, or in other words what the correct response should feel like (Seymour, 1954, pp. 66–7). Such techniques raise special problems for the two sets of data occur simultaneously instead of successively as in the normal knowledge of results situation. One consequence of the simultaneous presentation of extra "experimenter-supplied" feedback with "natural" feedback is that the former might not become redundant, for whereas an event may become predictable on the basis of preceding events, this clearly cannot happen with simultaneous events. Since no practical purpose is served by supplying extra data which the trainee cannot ultimately dispense with, the various possibilities inherent in this situation merit examination. The following experiments are concerned with the retention of a response subsequent to the withdrawal of informative feedback presented either simultaneously with the response or upon completion of the response.

Experiments

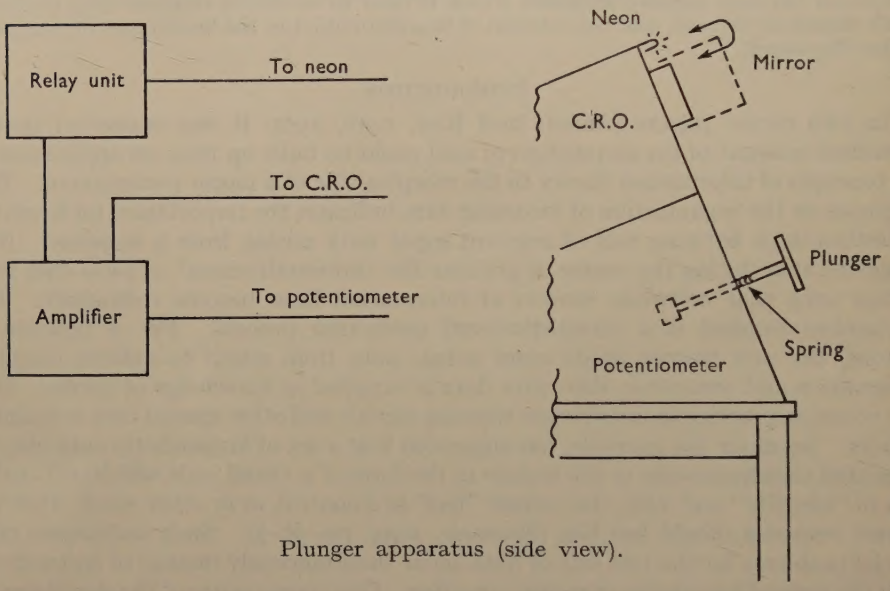
1st Set

Apparatus and Subjects

Figure 1 shows the general layout of the apparatus used in the first set of experiments. The situation might resemble instruction in the use of a hand power tool which involves the application of pressure to some rather delicate material. The subject stands at the

table grasping the plunger handle at waist height. The plunger is spring-loaded and pressure on it moves the cursor of a small potentiometer inside the box. Just below eye level but facing upwards at an angle of 30 degrees is a cathode-ray oscilloscope which is the subject's external display. A hinged mirror can be swung across the CRO face so as to screen it from the subject's view and at the same time to allow the experimenter to read the CRO. A neon bulb is also mounted on the CRO casing at about eye level. The change in potential caused by pressing on the plunger is amplified and deflects a spot of light on the CRO face. As the subject presses the light moves up the screen and returns to zero as pressure is released. The relay unit connected to the neon contains two thyratons which can be triggered at any preselected point, one switching the neon on at a given pressure and the second switches it off as a higher pressure is reached. The general plan of the experiments was to give a period of training under conditions of augmented feedback followed by test trials in which the augmented feedback was no longer present. Four conditions of presentation of supplementary information were used and in all 38 subjects were used.

FIGURE 1



Plunger apparatus (side view).

Experimental Conditions

All the subjects were instructed that they were to learn to exert a precise pressure on the plunger handle in 30 trials during which they would be given some extra information to help them and that after this practice they would have to go on trying to exert the same pressure for 70 more trials unaided. At each trial the subject was to press what he believed to be the correct pressure to hold that pressure for a moment and then release. Short rest pauses punctuated the tests at 25 trial intervals. The four conditions were as follows:

Condition A. Subjects could see during practice the CRO face and the light moving with pressure applied. The target pressure was clearly marked on the graticule so that the subject could know at any moment not only if his response was correct, but also the precise extent and direction of the error.

Condition A1. The screen was covered but the neon circuit switched on so that the light came on 1 degree below the target and was switched off 1 degree above the target. Thus the subject could know immediately whether or not his response was correct and the direction of the error but not its extent.

Condition B. The screen is again covered and the subject is given verbally a scale reading read off by the experimenter at the end of each push whilst the subject is still exerting the pressure. The graticule was divided into 1/10ths of an inch, the target pressure being at 20 in the middle of the screen.

Condition B1. The CRO was again covered for the application of the pressure, but the screen was removed at the point at which the subject held the pressure steady at the end of each push. Thus the subject could, with a short delay, match the applied pressure with the position of the light on the CRO.

The first group of 12 students, all undergraduate or post-graduate students of psychology, attended on three occasions, doing on each occasion either A or A1 or B, two subjects in each of six possible orders of presentation. The second set of 12 subjects attending on three occasions did conditions A, B and B1, two subjects being given one of six different orders of presentation. On each occasion a spring of a different strength was used and subjects were told that a new or different pressure was to be learned. On the first session the required pressure was 5 kg., on the second 1 kg., and on the third 10 kg. Fourteen members of a W.E.A. summer school, only one of them having anything to do with psychology, and of ages ranging from 25 to 65, attended once each. Eight did condition A and 6 condition B.

Results

Error was measured in 10th-inch deviations from the marked target on the CRO. The range of the scale was 0 to 40, with the target at 20. During the 30 learning trials, subjects under the A condition were able to and did in fact produce the correct pressure on each response, but on the B conditions fairly large errors were made which were gradually reduced so that a few precisely correct responses were made towards the end of the 30 trials. On the test trials on the B conditions, this improvement underwent a slow reversal, but by contrast the A conditions produced an almost immediate and drastic loss of accuracy. So much so in fact that 20 trials after the removal of the cue many subjects were making errors beyond the limits of the recording apparatus and the potentiometer was damaged several times. Table I

TABLE I

PLUNGER EXPERIMENTS

Table of Mean Error Scores
(First 20 Test Trials)

Subjects	*Condition*	*A*	*B*	*A1*	*B1*
12 Univ.	Mean	10·8	3·6		
	S.D.	5·1	1·5		
14 W.E.A. . .	Mean	14·0	4·4		
(8 A, 6 B) . .	S.D.	2·1	1·7		
12 Univ.	Mean			10·6	4·7
(6 A1, 6 B1) . .	S.D.			5·7	1·7

shows the general result in terms of mean error deviation in the first 20 trials only, since a large proportion of errors on subsequent trials were unscorable. Generally speaking, both the A conditions give about three times as much error as the B conditions and the differences are highly significant (Table I*a*). A1 does not give significantly less error than A, but B, verbal KR, is significantly better than B1, visual KR.

TABLE 1A

TABLE OF SIGNIFICANT DIFFERENCES

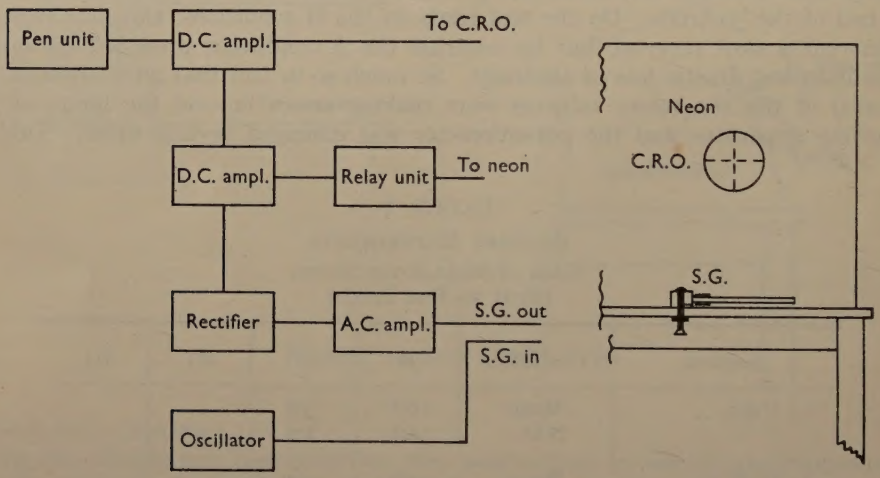
	"t"	p
A—B Univ. ..	4·58	< 0·001
A—B W.E.A. ..	22·71	< 0.001
A—A1 Univ. ..	1·36	< 0·2
A—B1 „ ..	5·41	< 0·001
B—A1 „ ..	6·08	< 0·001
B—B1 „ ..	3·86	< 0·01

2ND SET

Apparatus

For all the subsequent experiments a new piece of apparatus was used (Fig. 2). The plunger apparatus necessarily entailed an appreciable amount of movement and the potentiometer was rather delicate and was frequently damaged. These difficulties are considerably reduced by the use of strain gauges. A piece of aluminium alloy (35 cm. × 25 mm. × 8 mm.) was used, clamped at one end as in the diagram, and strain gauges mounted next to the clamp. Pressure on any part of the bar is registered.

FIGURE 2



Strain gauge apparatus (front view).

The metal bar is solid and clamped down, making the apparatus very much more sturdy than the plunger-potentiometer apparatus, and the movement involved in changes of pressure is very small; 1,000 gm. on the end of the bar moves it less than 1 mm.

The strain gauge bridge is fed by an A-F oscillator and the output from the bridge passes through a stage of A.C. amplification to rectifier to a D.C. amplifier. At this point a thyratron-operated relay unit can be led off to operate the neon signal lamp as in the plunger apparatus. A further stage of D.C. amplifications feeds both a pen recorder unit and the Y plates of the CRO display. The subject sits at the table to which the strain gauge unit is clamped and applies pressure to a specified point on the bar. The CRO is at eye level and can be concealed by a movable screen. Pressure on the bar is instantaneously registered on the pen recorder and by movement of the light spot on the CRO, thus the subject has immediate visual feedback from his response.

Subjects and conditions

Using the strain gauge apparatus, a batch of three experiments using 36 subjects in all was carried out. As before, comparison is made between the effects of different training periods on subsequent test trials, although this time 50 training trials were used, followed by 50 test trials.

Each experiment had three conditions and again subjects attended on three separate occasions. Two subjects did each of the six possible orders of presentation. All the experiments were concerned with variations on condition A.

The first experiment was based on the simple suggestion that in condition A any advantage afforded by the availability of extra and precise information might be nullified by the fact that subjects had no opportunity to practise on the test condition, that is, without the aid of the extra cue. Thus the test condition would be completely novel after a period of training where a helpful cue had always been present. The three conditions for this experiment were then, the standard condition A, new condition B in which subjects had alternate trials with the screen visible and "blind," that is, as in the test condition, and condition C in which five seeing trials were alternated with five "blind" trials. Thus in B and C the extra cue is available only on half the training trials.

The second experiment was an attempt to tackle the problem of how informative the extra cue was, with the hypothesis that perhaps it gave too much information, thus encouraging subjects to rely on it even though they knew that they would subsequently have to do without it. The conditions were devised with a view to reducing the target tolerance required by giving a big target and a much inflated light spot. Condition A was almost the same as above, except that the graticule was blacked out and the subject's visual data was a white target $\frac{1}{4}$ in. with a light spot of $\frac{1}{8}$ in. Condition B was a target of 1 in. in the middle of the screen with a light spot $\frac{3}{4}$ in. in diameter and condition C was a target of 2 in., the middle 2 in. of the 4-in. screen, again with a large light spot. Subjects were carefully instructed to use the light as little as possible, but just enough to guide them in getting into the correct area. They were instructed not to aim specifically at the top, bottom or middle of the target area.

The third experiment deals with the simple hypothesis that condition A results in poor learning because the subject who only practises the correct response has no chance to sample a range of possible responses, in other words doing the wrong thing might be a necessary condition of learning to do the right thing. This may be especially true of this kind of task where the correct response is not a discrete thing in itself but involves a judgement and perhaps a comparison of a range of stimulus values. Condition A was again used and for condition B subjects were required to press alternately at half the required strength and at half as much again, precise pressures being indicated on the screen. It was made clear to the subjects that they were practising these two pressures in order that they should be able to make a comparative judgement and in the test trial press a little harder than the lighter pressure and a little softer than the heavier pressure, or in other words half-way between the two. In condition C, subjects were asked to practise pressing alternately to any point at which the spot of light was still below the centre line and to any point above the centre line. It is important to remember that in all these experiments the subjects were given clear instructions that it was the feeling of pressure which they must attempt to learn and that in the second half of the experiment they would have to manage without any extra cue.

Results

Results are summarized in Table II. The mean error scores are derived from the first 25 test trials after removal of extra cues for the reason that whatever the pressure used, 500, 750 or 1,000 gm., many subjects produced errors greater than 100 per cent. and it was not possible to measure them accurately. Table II gives the mean error score for 12 subjects in each of the three experiments. There are six possible orders of presentation of the three conditions. A first, B second, C third; A first, C second, B third, etc. Two subjects were used for each of these six orders. The matrices in Table II are set out to compare the three conditions with the three ordinal positions of each condition. Thus cell 1A is the mean error score of those four subjects who did condition A on the first session and cell 3C is the mean error score of those four subjects who did C on the third session.

TABLE II

THREE EXPERIMENTS WITH STRAIN GAUGE APPARATUS

Table of Mean Error Scores

Experiment I. Intermittent feedback

Order of presentation	*Conditions*			
	A	*B*	*C*	*Mean*
1	10·39	7·4	7·25	8·35
2	7·72	4·43	7·14	6·43
3	5·85	4·42	6·24	5·5
Mean ..	7·99	5·43	6·88	

Experiment II. Target tolerance

Order of presentation	*Conditions*			
	A	*B*	*C*	*Mean*
1	10·94	11·53	8·97	10·48
2	6·83	4·41	9·41	6·88
3	11·16	3·95	4·44	6·52
Mean ..	9·64	6·63	7·61	

Experiment III. Response variability

Order of presentation	*Conditions*			
	A	*B*	*C*	*Mean*
1	9·56	9·22	4·79	7·86
2	5·63	7·72	3·91	5·75
3	4·86	6·93	7·32	6·39
Mean ..	6·68	7·95	5·36	

In experiment I inspection of the means give some indication that in the first session conditions B and C give *less* rather than more error than A. This is of some interest in so far as "feedback" or "reinforcement" is given on only half as many trials. This is not firmly established since the first row represents only four subjects in each condition, but it is the reverse of what might be the expected trend. Overall comparisons by analysis of variance in which each subject is represented in each condition and each ordinal position (Table II*a*) shows that the effect of conditions is not significant with a p value of only 10 per cent. Order (i.e. first, second or third session) is significant at the 5 per cent level. Inspection of the mean shows that error on each of the three conditions is reduced by prior practice on the other conditions, but the benefit to A of being given last is greater than that to B or C. The interaction of condition and order is not significant and the differences between A and B, and B and C are not reduced by practice, but the mean difference between the 100 per cent. feedback condition and the two 50 per cent. feedback conditions is reduced from 3·06 on the first presentation to 0·96 on the third presentation.

TABLE IIA

THREE EXPERIMENTS WITH STRAIN GAUGE APPARATUS

Table of Analysis of Variance

Experiment I. INTERMITTENT

	S.S	*D.f.*	*M.S*	*F*	*P*
Subjects ..	45,595	11	4,145	0·982	N.S.
Conditions ..	24,897	2	12,448	2·949	10 per cent.
Order	31,542	2	15,771	3·736	5 per cent.
Interaction ..	11,463	4	2,865	0·679	N.S.
Residual ..	67,547	16	4,221		
Total	181,044	35			

Experiment II. TOLERANCE

Subjects ..	174,513	11	15,864	6·646	0·1 per cent.
Conditions ..	35,455	2	17,727	7·426	1·0 per cent.
Order	71,946	2	35,973	15·070	0·1 per cent.
Interaction ..	8,186	4	2,046	0·857	N.S.
Residual ..	38,204	16	2,387		
Total	328,304	35			

Experiment III. VARIABILITY

Subjects ..	170,305	11	15,482	3·220	5 per cent.
Conditions ..	25,354	2	12,677	2·637	20 per cent.
Order	17,466	2	8,733	1·816	20 per cent.
Interaction ..	11,459	4	2,864	0·596	N.S.
Residual ..	76,939	16	4,808		
Total	301,521	35			

In the second experiment inspection of the means shows that the widest tolerance, condition C, gives slightly less error than the other two. Error on B is considerably reduced when given third and the same is true of C to a lesser extent. A is a little worse when presented last. The effect of conditions is significant at the 1 per cent. level and the effect of order of presentation is significant at the 0·1 per cent. level as is the variance due to subjects. Observation of the subjects and their introspections suggests that two different methods of doing the task were employed and this depended partly on the condition and partly on the amount of practice. Normally subjects press the bar and watch the light simultaneously, but in the wide tolerance conditions they were able to press the bar within the tolerance limits without looking at the light, but checked its position immediately afterwards. Use of the latter method favours a much lower error score and the scatter of scores is much greater on the B and C

conditions and also on the second and third sessions. It is obviously easiest to learn this method on the wide tolerance and it is difficult to apply the method on the narrowest tolerance, it thus seems reasonable to infer that the greater improvement of B from first to third in order of presentation is due to the previous practice on C. In experiment III inspection of the means 1A, 1B and 1C suggests that the wider tolerance in C results in slightly better performance whereas there is little difference between A and B, both having a narrow tolerance but differing in the extent to which the range of pressures was sampled. Overall analysis, however, shows that the effects of condition and order were both insignificant with a p value of 20 per cent., but the subject variance is significant at the 5 per cent. level. A seems to benefit rather more than B by being given last and C gets worse. Since it was observed that some subjects picked up the method of successive attention to the kinaesthetic and visual cues as in experiment II any difference which might exist is probably due to the use of this method. It is not surprizing that C is worse when done last since subjects would perhaps tend to use the simultaneous attention method having had to use it on the two previous sessions. This inconsistency resulting in the insignificance of order and condition effects is in keeping with the adoption of a particular method of doing the task, by some subjects, in condition C.

3rd Set

Subjects and conditions

In all the foregoing experiments the error tends to be large and positive when measured in scale units of the display used. A loophole in the experiments is that a different pressure had to be used for each session since it was much more difficult to get a different subject. The actual pressure was tied to the order of presentation, thus all subjects learned the medium pressure first, the light pressure second and the heavy pressure third. In order to substantiate the impression that error was related to the visual scale rather than to the actual amount of pressure used, and thus that the linking of order with particular pressures is of no importance, the following experiment was attempted. Nine subjects were given condition A on three occasions, each time with a different pressure, each subject being allotted to one of the six possible orders of presentation of these three. Due to purely technical reasons only nine of the proposed 12 subjects could be tested. The results, however (Table III), show that none of the differences in error between pressures are significant when error is measured in terms of the visual display scale. Over-estimations of the same order of magnitude occur whatever pressure used, thus no serious disadvantage results from having pressure tied to order of presentation.

TABLE III

Strain Gauge Experiments

Error Scores of 9 Subjects, Condition A, 500 gm., 750 gm., 1,000 gm.

		Differences		
500 gm.	Mean Error = 8·08	500–750	"t" = 0·57	N.S.
750 gm.	Mean Error = 8·88	500–1,000	"t" = 0·67	N.S.
1,000 gm.	Mean Error = 7·88	750–1,000	"t" = 1·33	N.S.

The last experiment concerned the extent to which the extra cue was used to control the ongoing response and 12 subjects were given condition A with the difference that they were required to tap rather than apply a steady pressure to the bar. This condition was compared with the giving of verbal feedback in terms of "too hard," "too soft" and "just right" immediately following each response in the training trials. The "just right" response corresponded to + or − 1 degree on the paper record or + or − 2 degrees on the screen which the subjects could not see. Twelve subjects were tested, half having visual feedback and half verbal feedback first. All the subjects learned 500 gm. followed by 1,000 gm. on each of the two sessions.

TABLE IV

Error Scores of 12 Subjects Tapping at 500 and 1,000 gm., Immediate Visual Feedback and Verbal Feedback

		500	1,000
Visual	1st 25	1·4	1·49
	2nd 25	1·96	1·94
Verbal	1st 25	0·6	0·66
	2nd 25	0·86	0·22

The only significant difference between visual and verbal is in the 2nd 25 and 1,000 gm.

Results

The results can be seen on Table IV. Errors were far smaller than with other A conditions, with very little difference between the visual and verbal feedback. The table shows the average error above the target, but since some of the errors were under-estimates, the means were compared by "t" test of the total magnitude of the response. This is a little more realistic perhaps when the error is so small, since it shows that the total amount of pressure applied was not significantly greater except towards the end of the test at the heavy pressure.

A proprioceptive illusion

There is one more result which, for all that it is an observation rather than an experiment, is probably the most significant. As has already been pointed out, condition A invariably produces gross errors of over-estimation. Many, but by no means all, subjects make two remarks about this condition. The first is that right at the beginning of the test trials they feel "lost" and the second is that the bar feels much stiffer in the absence of the visual display. "It is as though something had happened to the bar, it felt resilient when I was watching the light, but when the light was covered up it felt stiff."

During the initial setting up of the apparatus we had some trouble with the amplifiers and quite by accident got a condition where the light spot responded linearly to pressure but stopped suddenly half-way up the screen. The effect on the operator is most striking. As the light stops moving, the metal bar appears to stop moving. There is a strong impression that a mechanical stop has been introduced and the impression can only be described as a sudden jolt felt in the fingers. The effect is present in both experienced and naïve subjects and appears to be relatively independent of factors such as the amount of practice and set. Whilst the effect is less striking if the moving light simply passes behind a screen, there is no doubt that the bar feels stiffer generally without the light than with it.

Summary of Results

(1) When Knowledge of Results is provided in a fairly precise form of scale readings given either verbally or visually upon completion of the response, subjects can learn to apply a fairly precise pressure and the ability to reproduce this pressure declines gradually over a number of trials after KR is no longer applied. There is only a slight difference between older and younger subjects.

(2) If a supplementary cue (either a single light indicating the correct pressure or a moving light giving precise and immediate error indication) is present, subjects

still perform accurately during training trials, but on removal of the supplementary cue subjects become immediately less accurate and within a few trials are pressing at about twice the required pressure.

(3) If the supplementary cue is present only on 50 per cent. of the training trials either on alternate trials or alternate blocks of five trials, the error in the test trials is reduced.

(4) If target width is increased, error tends to be less on removal of the supplementary cue, but this is attributable to a change in method in some but not all subjects.

(5) If training consists of practice with a range of pressures, the error is less on removal of the supplementary cue only if the precision of the required response is also reduced.

(6) The error on removal of the supplementary cue tends to be related to the visual scale rather than to absolute pressure used. Thus a pressure of 500 gm. will be over-estimated by about 500 gm. and 1,000 gm. will be over-estimated by 1,000 gm.

(7) Where the required response is a ballistic tap as opposed to a steadily applied pressure, the supplementary cue produces no better and no worse retention than knowledge of results in terms of "too little," "too much" and "just right," except towards the end of unaided practice with a heavy pressure.

(8) On removal of the supplementary cue, subjects (especially where the strain gauge was used) characteristically report (*a*) that they immediately feel lost or confused, and (*b*) that the bar has "gone stiff."

(9) In the presence of the moving light, the strain gauge bar is estimated to move more than it actually does. If the moving light is suddenly stopped whilst the subject is pressing on the bar, there is a strong impression that a mechanical stop has been introduced in the bar. The impression of sudden change in the flexibility of the bar appears as a jolt felt in the fingers.

Discussion

The results of these experiments give considerable support to the contention that the perceptual aspect of a task is of over-riding importance in learning. To repeat a precise response is not a sufficient condition for learning that response even if the response is known immediately by the subject to be correct.

The literature contains precedents for this finding (Carr, 1930; Seashore, Underwood and Berks, 1949; Houston, 1947; Lincoln, 1954) which have been summarized by Miller (1953). The A conditions by providing simultaneous feedback during the course of the response not only give extra information about the response but also permit its modification. For this situation, Carr has used the term "guidance" and Miller the term "action feedback." The reasons why guidance or action feedback is detrimental to learning are far from certain, although a number of plausible suggestions may be made. It could be asserted, for example, that "negative" practice is essential, because the subject is not simply trying to learn "a response" but rather to distinguish this response from a class of similar responses. However, experiment 3 (second series) reduces the plausibility of this explanation at least for the present situation.

Miller puts forward the suggestion that trainees will be tempted to rely on "good" cues and will naturally perform badly when these are removed. In this way good

performance during practice is incompatible with good performance on criterion trials and trainees, he suggests, should be motivated to learn rather than simply motivated to get high scores during practice. In the present study there was little sign of this attitude on the part of the subjects. They all knew that they would have to rely solely on the kinaesthetic cue and were attempting to learn the correct "feel." It would appear from the present study that factors other than set induced by instructions are operating.

A third possibility is the tentative hypothesis that the redundancy of some cues relevant (initially) to a skilled performance will depend on the subject's ability to predict them and that this will occur if the relevant cues are serially ordered but not if they occur simultaneously. The result lends some support to this hypothesis, which is in line with the evidence on the difficulty of establishing simultaneous and backward conditioning (Wolfe, 1932; Kimble, 1947; Fitzwater and Reisman, 1952). Further indirect support for this hypothesis is found in the result of experiments 2 and 3 (second series). In these instances a wide target tolerance during the training trials enabled some, but not all subjects to adopt the method of successive attention to the two sets of relevant cues and this resulted in better retention on removal of the extra cue. This result suggests a parallel with Broadbent's work on two channel listening (Broadbent, 1957) though in his experiment the two channels were carrying independent messages and in the present experiment the two channels carry information which is closely correlated.

The simplest hypothesis which might be advanced to account for poor retention in the A conditions is that subjects were simply unable to attend to the two sets of cues simultaneously, but here again there are difficulties, for the evidence might equally well support the hypothesis that simultaneous attention can and does occur, but that this results in an interaction of the two sets of cues. If subjects perform poorly on withdrawal of the visual cue simply because they have been attending *solely* to it and had, therefore, no experience of the "feel" of the bar one could not predict the direction and extent of subsequent errors. If a subject is asked to estimate something which he has never experienced his estimates might not be random, he might make a guess and repeat this estimate, but one could hardly expect that all subjects would make errors of approximately the same extent and in the same direction. The fact that they do so suggests that subjects are estimating something which they *have* experienced, but that this experience has been systematically distorted. The subject's remarks and introspections add some support to the latter interpretation. All subjects could apparently feel the bar in the presence of the visual cue and in fact often reported that when the visual cue was removed, the "feel" of the bar changed. There is also a theoretical reason for rejecting the attention hypothesis. As was suggested in the previous paragraph, the difference between the present case and the normal two channel listening situation is that in the latter, two independent messages are involved. Here, however, there is a one-to-one correlation between the visual and the kinaesthetic data. A source of information, as distinct from the locus of a message, is a single source of variation, thus the same message can be presented on different modalities and different messages can be presented on the same modality. As Broadbent has pointed out, difficulties of attention arise rather in the case of two independent sources (in this sense) than in the case of two independent modalities, thus we would not in this case expect a conflict of attention. The protocol strongly suggests that the two simultaneously presented sets of data interact such that the kinaesthetic stimulation is distorted during the training trials. A large body of literature on sensory interaction (Ryan, 1940; Gilbert, 1941; London, 1954; Symons, 1954) contain examples of both facilitatory and inhibitory intermodal effects.

It might be suggested that the presence of the visual stimulus inhibits pressure sensations, thus on removal of the visual stimulus pressure, sensations are fully restored and the bar becomes stiff. However, if this were the case, under-estimations of the required pressure would be expected. A facilitatory interaction fits the facts more closely. In the presence of the visual stimulus, the bar appeared to move through a greater distance than was the case. When the visual stimulus is withdrawn, subjects have to exert more pressure in order to reproduce the same apparent movement. This intensifies the concomitant pressure sensations, and hence the bar feels stiffer. The subjects were confused by the contradiction between pressure and movement sensations, and, despite instructions to reproduce pressure, tried to reproduce the same apparent movement. It is a matter of some interest that the effect occurs, though to a lesser degree, when the visual cue is a stationary light, thus we cannot conclude that movement in the bar is simply "inferred" from movement in the visual stimulus. The evidence would appear to support the hypothesis that the two simultaneously presented sets of data interact such that movement sensations are facilitated. This interaction would appear to follow the correlation of the two sets of data. Ryan, in reviewing intersensory effects, remarks that much of the work concerns interaction between neutral stimuli and points out that the co-operation of correlated stimuli offers a more realistic and more interesting field of study. If the present results do indicate a genuine intersensory effect, it would appear to be of the latter kind.

Mention should be made of Grindley's finding (Dees and Grindley, 1951) that overshooting is greater the shorter the interval between trials. The tentative explanation was based on the interval between trials, but it is also the case that the feedback signal, the "result," must follow the response very quickly when the time interval is as short as 1·8 sec. This condition approaches closely the immediate feedback in the present experiments, and it might be suggested that overshooting is due to this factor rather than simply due to the spacing of the responses alone. It has been argued elsewhere (Annett and Kay, 1957) that Miller's distinction of action feedback and learning feedback might break down in rapidly succeeding trials, and it is certainly the case that the more closely trials follow each other, the more pronounced are the characteristic features of action feedback, namely rapid learning and rapid "forgetting." This question cannot be considered satisfactorily settled, however, in view of the finding in the tapping experiment, where the visual cue was virtually immediate but was not used for controlling the response. Little difference was found between ordinary knowledge of results and the use of the immediate visual cue. It may be noted in passing that the optimal time lag between UCS and CS in conditioning experiments is generally agreed to be of the order of 0·5 sec. and that the time involved in the tapping experiment between the initiation of the movement and its result, must be considerably less, perhaps of the order of no more than 0·05 sec.

In conclusion, the original hypothesis about the timing of feedback is sustained in so far as the withdrawal of simultaneous feedback results in a much greater loss of accuracy than withdrawl of feedback which has been delayed. Furthermore, retention of the response is better where the subject can deal with simultaneous data successively, and subjects tend to adopt this method wherever conditions allow them to do so.

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ON THE REDUCTION OF CHOICE REACTION TIMES WITH PRACTICE

BY

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Information theory concepts have been discussed in relation to data from choice reaction time experiments. Specifically, it has been stated that choice reaction time is proportional to the logarithm of the number of randomly appearing alternative stimuli. It is suggested that the reported increase in choice reaction times with an increased number of alternatives is the result of insufficient practice. Data are provided to show that, with sufficient practice, this increase does not occur between two and four choices.

Introduction

In recent years there has appeared a revival of interest in choice reaction times, brought about in large measure by the attempted application of communication theory techniques to the analysis of human behaviour. Bricker's excellent review of pertinent research states the case for the information theorist in a convincing manner (Bricker, 1955). For the purposes of the present discussion, the relevant material in that review concerns the allegation that disjunctive or choice reaction time is directly proportional to the information contained in the stimulus. The first to point this out was Hick (1952) using his own data and data collected by Merkel (1885) many years previously. Hyman (1953) and Crossman (1953, 1955) have subsequently provided supporting data. All of these researches show, in general, that choice reaction time is proportional to the logarithm of the number of randomly appearing alternative stimuli. The most general hypothesis generated from a consideration of these experimental results is stated by Bricker (1955) to be, ". . . that average reaction time, unless otherwise restricted, is an increasing linear function of the average amount of information transmitted from the display to the response."

This hypothesis can be useful, as stated, only if choice reaction times do not change substantially with practice. Hick states that he found very little improvement with practice. However, data from Merkel that he cites, collected 65 years earlier, do exhibit practice improvement. Hick suggests that this improvement may have been due to easily learned presentation sequences. It may also have been due simply to practice. Woodworth (1938, p. 331–2) states that the disjunctive reaction time becomes quicker with practice, but that there is no evidence that it ever becomes as rapid as the simple reaction time. He cites nothing but very ancient data to support this position. More recently, short-term improvement of choice reaction times with practice has been demonstrated by several researchers, while Leonard has produced data that exhibit marked improvement with practice over a rather extended period of time (Klemmer and Muller, 1953; Leonard, 1954; Alluisi and Muller, 1956). Table I shows how Leonard's reaction times for three choices and for six choices decrease with practice, and more importantly, how the difference between the two conditions of choice decreases in an almost linear fashion as practice increases.

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Leonard concludes from these data that a clear difference exists between reaction times for six choices and for three choices after extended practice. What is not so clear, however, is whether this difference would still exist if practice were extended until reaction times for the two conditions of choice had ceased to decline.

Superficially it might seem that learning to move one of ten fingers in response to a signal is not a task that would require a great deal of training. It has often been pointed out in the past, however, that the execution of even simple skills continues to improve with practice over periods of months and sometimes years (Woodworth, 1938, p. 156; Wolfle, 1951). More recently Crossman (personal communication) has reported that the time taken by operators in an industrial plant to perform a relatively simple, manual operation involving a decision was still decreasing after a million repetitions.

In view of these considerations it seems unlikely indeed that choice reaction times are immune to significant reductions with extended practice. It is even conceivable that with enough practice there is no such thing as a *choice* reaction time, but that choice reactions are reducible to the basic reaction time regardless of the number of alternatives provided.

Apparatus and Procedure

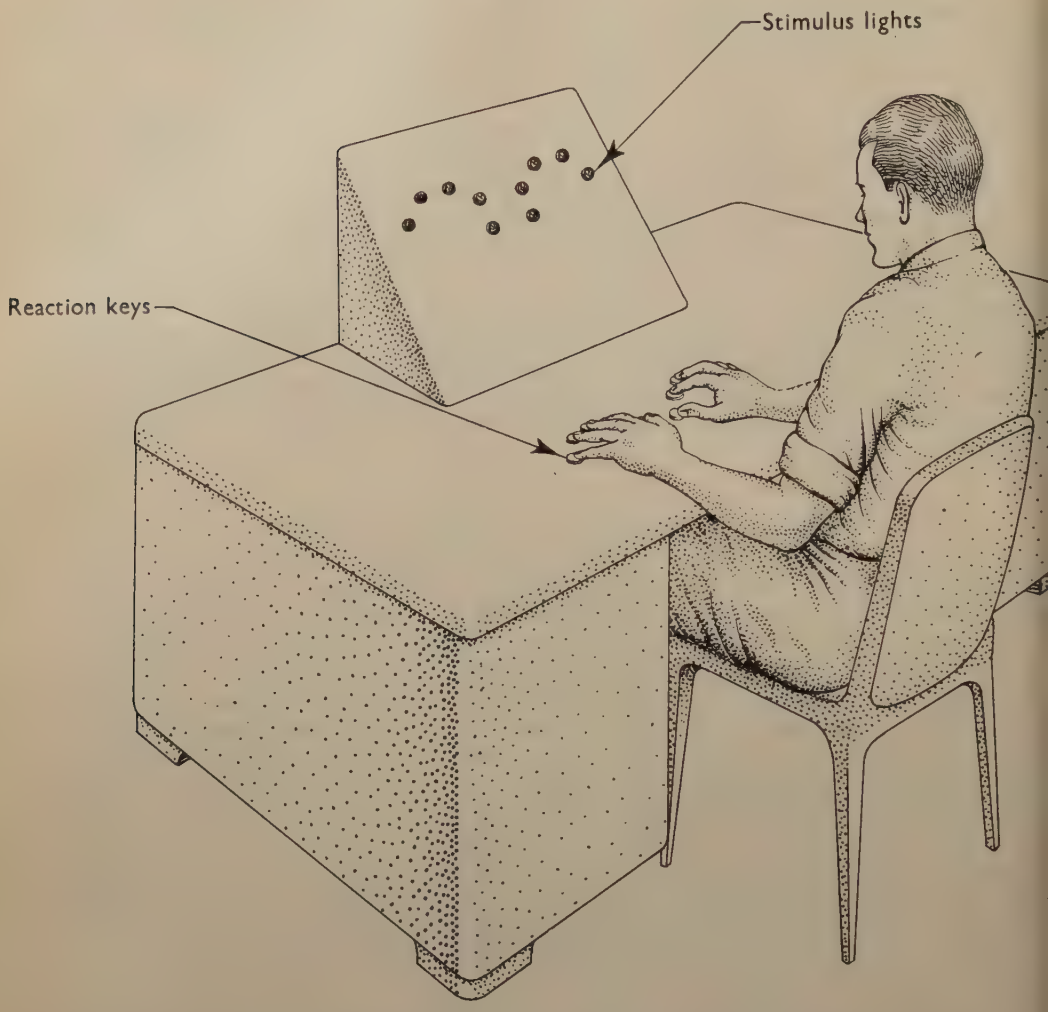
The equipment used in this experiment was similar in many respects to that used by Hick in his choice reaction time experiments. Stimulus lights were small pea-lamps activated by a teletype tape transmitter according to a binary code punched in the tape. The tape transport mechanism was programmed to present a new stimulus every 5 seconds. The response keys were round, plastic buttons about $\frac{1}{2}$ in. in diameter with a slightly concave top. They operated normally-closed, spring-loaded microswitches. A force of 5 grams applied vertically was sufficient to actuate the switches. The wiring circuit was so arranged that when any one of the stimulus lights was energized, depression of the correct response key extinguished the light. Speed of reaction or the reaction time was measured by a Standard Electric Timer, Type S–1, activated by the stimulus light circuit and stopped when the normally-closed microswitch, associated with the correct response button, was opened. Reaction times were recorded to the nearest 10 milli-seconds.

The physical layout of the equipment is illustrated schematically in Figure 1. The stimulus lights were positioned on the display panel in accordance with the arrangement of the response buttons, except that they were approximately 45 deg. from the vertical. They were each about 0·25 in. in diameter and were much closer together than the response buttons. The line of sight distance from the subject to the display lights was about 30 in., so that eye movements were virtually unnecessary.

While the apparatus described above is capable of handling up to ten choices, the present experiment was concerned with only two different conditions—namely, reaction times to two choices and to four choices. For these conditions, the index and second finger of each hand were used. For the two-choice condition, the two index fingers were used, while for the four-choice condition all four fingers were, of course, required.

For the two-choice condition, stimulus sequences were 200 stimuli in length, whereas for the four-choice condition they were 400 stimuli in length. Initially, five tapes were made for each condition, each with a completely different sequential order. The order of the stimuli on all tapes was established by drawing lots with two restrictions imposed. In the first place, equality of stimuli was maintained; so that for two choices, 100 responses were required of the index finger of the left hand and 100 responses were required of the index finger of the right hand. The second restriction relates to the number of times that a given stimulus was allowed to appear without a different stimulus intervening. In all cases this number was limited to

FIGURE 1



A view of the reaction time apparatus used in the experiment.

three. Although the subject was not informed of this latter restriction, it was assumed that he became aware of it during the course of the experiment. Therefore, in analyzing the data from the two-choice condition, all responses following a sequence of three like stimuli were disregarded. This precaution was not deemed necessary for the four-choice data.

In spite of the care taken in constructing the stimulus tapes and seeing that the subject had no knowledge of which tape was being presented, it was considered possible that some learning of the sequences could take place. To test this possibility, five new tapes were constructed for each condition and were substituted for the original ones about half-way through the experiment. Analysis of the data from the new tapes provided no evidence whatsoever that any learning had occurred.

The experiment was divided somewhat arbitrarily into 15 replications, or trials. Each replication consisted of one run-through of every tape for both conditions. This represents a total of 3,000 reaction times per trial, or 45,000 reaction times for the whole experiment. The two-choice and four-choice conditions were alternated throughout, and all stimulus sequences for the two conditions were introduced by a short practice period.

TABLE I

MEAN CHOICE REACTION TIMES AND NUMBERS OF REACTIONS FOR THREE CHOICES AND SIX CHOICES AS AFFECTED BY PRACTICE. TIMES ARE IN MILLI-SECONDS. (After Leonard, 1954)

Amount of practice		*Number of choices*		*Difference: 6 minus 3*
		3	6	
May	RT	261	323	62
	N	1,152	575	
September	RT	268	319	51
	N	576	576	
December	RT	249	294	45
	N	1,152	1,104	

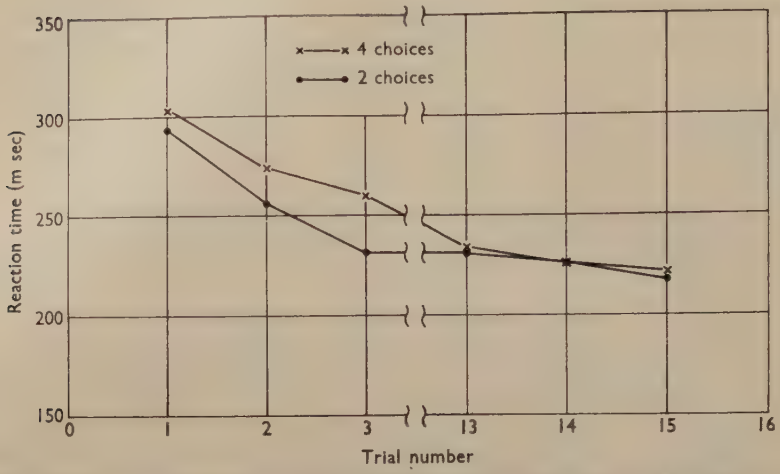
Only one subject was used for the entire experiment. He was a young college student—22 years of age—who had participated previously in a rather lengthy choice reaction experiment involving as many as 20 degrees of choice. He had no knowledge of the aims of the research under discussion. He was a very conscientious and co-operative subject and was given encouragement frequently to keep his motivation as high as possible during this exceedingly tedious task. His instructions were to react as quickly as possible to all stimuli, making as few errors as he could. Frequent rest periods were given and no single experimental session lasted longer than one hour.

In analyzing the data, only the reaction times from the index finger of the left hand were used. Each condition, then, provided a total of 7,500 reaction times—or a total grand total of 15,000, from which were subtracted all errors and all reaction times for stimuli following three identical presentations in the two-choice condition. Errors averaged on the whole less than 2 per cent. throughout the course of the experiment, and were omitted from the calculations of mean reaction times.

RESULTS

Figure 2 shows a plot of the mean reaction times for two choices and for four choices as a function of the trial number. Only the first three and the last three trials have been plotted since what happened in between is simply more of the same. Each data point in Figure 2 represents a mean of approximately 500 reaction times. There are two things of immediate interest to be noted. First and foremost is the fact that the difference in reaction times between two choices and four choices after 13 trials is to all intents and purposes reduced to zero. Of great interest also is the fact that reaction

FIGURE 2



Mean choice reaction times for two choices and four choices as a function of the number of trials.

times were still decreasing slightly after 15 trials. This was true, at least for the four-choice situation, but for two choices it seems possible that an asymptote was reached on the third trial. It would have been worthwhile, perhaps, to pursue the matter further, but unfortunately the end of the school term interrupted the research. The experiment was begun in January with the thought that the period from January through May would be sufficient to show whatever result was to be shown. In one sense it was, but in another sense a longer experiment would have been useful in showing the number of reactions required for no further reduction in reaction times to occur. That reaction times were still decreasing after so many thousands of reactions is significant also in view of the fact that the subject was a highly practiced one when the experiment began. This is evidenced in part by the mean difference between reaction times for two choices and for four choices on the first trial, shown in Figure 2. The difference here is only some 10 milli-seconds. Practice reduces the times for two-choice reactions at a faster rate than it does for the four-choice reactions, as can be seen by the difference between the two conditions on the third trial. This difference is almost 30 milli-seconds.

TABLE II

MEAN CHOICE REACTION TIMES AND STANDARD DEVIATIONS FOR TWO AND FOUR CHOICES AS AFFECTED BY THE NUMBER OF PRACTICE TRIALS. TIMES ARE IN MILLI-SECONDS

Number of choices		*Number of trials*					
		1	2	3	13	14	15
2	Mean	293	255	232	232	226	218
	σ	64·5	65·1	52·4	36·3	35·9	33·6
4	Mean	303	272	261	234	226	222
	σ	71·2	54·0	53·3	46·8	37·3	34·7

Another result worth mentioning is shown in Table II, where the mean reaction times and standard deviations are given for the data plotted in Figure 2. It is evident that the standard deviations display the same tendencies as the mean reaction times. Thus, after 15 trials and many thousands of reactions, the variability of the subject was also still decreasing.

TABLE III

PER CENT. CHOICE REACTION TIME ERRORS FOR TWO CHOICES AND FOUR CHOICES AS A FUNCTION OF AMOUNT OF PRACTICE

	Number of choices	
Trial	2	4
1	1·5	0·8
5	1·6	0·6
10	1·0	0·6
15	0·3	0·4

Table III shows the percentage of errors committed throughout the course of the experiment. Errors, in this case, consisted entirely of inappropriate responses to the light stimulus and, as might be expected, tended to diminish in number as responses become more and more practiced. Thus as variability of response decreases, the tendency to make mistakes decreases likewise.

DISCUSSION

It seems fair to say, in view of the results presented, that choice reaction times are significantly affected by practice. Furthermore, strong evidence has been advanced to support the thesis that with enough practice there may be no increase in choice reaction times as the number of alternative stimuli is increased. This at least seems to be true enough when the number of alternative stimuli is increased from two to four. There appears, on the face of it, no reason why the same might not apply for any reasonable number of alternative stimuli provided both time and energy to pursue the matter are available.

Ultimately there must come a point in choice reaction time experiments when a lifetime is not long enough to provide all of the practice necessary to reach a physiological limit. But for that to happen it is likely that the stimulus display system and the response system would have to be so physically large and complicated that many other factors not directly related to reaction times *per se* would intervene. Such things as gross eye movements and gross limb movements come to mind. For the purposes of this discussion it has to be assumed that such factors as these are negligible.

Leonard's (1954) data, while inconclusive by themselves, tend to suggest that the difference between six choices and three choices might disappear with enough effort. Leonard himself, was led to remark that the difference between six choices and three choices, was smaller than data available at the time had led him to expect. He was prepared, on the basis of this finding, to doubt Hick's constant rate of gain hypothesis when a much more fundamental doubt should have arisen; namely, that the basic

assumptions underlying the whole scheme were shaky. Unless reaction time increases with increasing alternatives of choice, there is no case to be made for the use of information theory concepts in the analysis of choice motor responses of this sort. To be sure, as Hick and others have shown, data from subjects who have not overlearned their task do fit a logarithmic function when number of alternatives is plotted against reaction time. But, as Cronbach (1955) has pointed out, a curve fitted by a logarithmic function can always be fitted by a polynominal and by some other relations as well.

Bricker, in the review quoted earlier, cautions against the general use of the expression, $RT = a + b \log N$, to predict choice reaction times since so many factors of known importance are omitted. He lists such things as stimulus discriminability, response difficulty and stimulus response compatibility, but there are many other as well—not to mention the serious effect of extended practice. All of this serves to point up a fact that has been known for some time. Disjunctive reaction times are exceedingly complex and any predictive equation, to be valid, must include a great many more factors than just the number of choices available to the responder.

There has been a suggestion in the past that errors tend to increase with practice in choice reaction time tasks (Leonard, 1954; Deininger and Fitts, 1955). Klemmer (1954), however, provides information to the contrary, and this experiment has shown that errors certainly do not increase with increasing practice but may, in fact, decrease. Any other result would seem to be logically and experientially difficult to justify.

In conclusion, let it be made clear that this is not intended as a condemnation of the use of communication theory in the analysis of human behaviour. The intent rather is to point out some of the shortcomings associated with considering the human as analagous with a collection of electrical and mechanical elements whose design characteristics are inflexible. The human observer is adaptable—just how adaptable is in most cases unknown since very rarely is he extended to the limit. However, before any quantitative generalizations about his behaviour can be made, his limits must be carefully delineated.

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VISUAL DISCRIMINATION OF SHAPE BY OCTOPUS

Circles and Squares, and Circles and Triangles

BY

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An investigation of the relative discriminability of circles and squares, and circles and triangles for octopus is described. The main findings were: (1) The three pairs of figures, circle and square with base horizontal, circle and square with base at 45° to the horizontal, circle and equilateral triangle were equally discriminable for the octopus. (2) Complete transfer occurred to larger and smaller figures. (3) No transfer occurred from a square in one orientation to a square rotated through 45°, nor did training with the square in one orientation lead to any saving when the discrimination was relearned with the square in the other orientation. (4) No transfer occurred from a square to a pentagon. (5) The method of training with reward on every positive trial leads to quicker initial learning of a discrimination than training with reward only where the positive figure is attacked, but discriminatory performance with the two methods approached the same asymptote. The results are in agreement with findings with other animals where these are available for comparison.

Introduction

The primary purpose of the experiments to be reported in this paper was to explore further the capacities of *Octopus vulgaris* Lamarck in discriminating visually presented shapes. In particular, the relative discriminability of triangles and circles, and of squares and circles was investigated and an attempt made to discover the properties of the shapes in terms of which the octopus was discriminating by means of transfer tests. In addition, a comparison was made of the relative efficiency of two different training methods in order to serve as a basis for further experiments. The implications of the findings will be examined in the discussion.

The relative discriminability of squares and circles, and of triangles and circles is of some theoretical interest since it is often assumed that animals have more difficulty discriminating the former pair of figures than the latter (e.g. Deutsch, 1955), though the only evidence for this comes from rats.

Experimental Methods

Subjects

The subjects were 14 *Octopus vulgaris* Lamarck. They were obtained in the bay of Naples, and weighed 250–500 gm. when brought in from the sea. For details of methods of catching octopuses and their habits *v.* Boycott (1954).

Pre-training

Octopuses were kept in individual tanks, at one end of which a "home" was constructed on the floor. Subjects were given three days to adapt to the tanks, two days learning to take pieces of sardine, and two days learning to attack the positive shape before discrimination training began. For further details of the pre-training procedure *v.* Sutherland (1957a).

Training

Shapes were presented singly to the octopuses (successive discrimination training). Shapes were presented to them at the end of the tank away from their home in the frontal parallel plane and about 80 cm. from their eyes. Shapes were inserted into the water on the end of a transparent perspex rod held by hand; the shapes were moved up and down through a distance of about 3 cm. three times a second while in the water. The shapes were cut from opaque white perspex $\frac{1}{4}$ in. sheet. The shapes used in the initial training

were all 25 sq. cm. in area. Animals were trained to attack one shape (the positive shape) and not to attack another shape (the negative shape). If an animal attacked the positive shape within 30 seconds of its entering the water, it was rewarded with a piece of sardine weighing 6 gm. If an animal attacked the negative shape within 30 seconds of its entering the water, a 10-volt A.C. shock was delivered to its mantle by means of a pair of electrodes on the end of a probe. All animals received ten training trials (five positive and five negative) a day for 12 days. In the training of group I on any positive trial on which an animal failed to attack within 30 seconds, it was presented with a piece of sardine shown together with the shape, at the end of 30 seconds and an attack invariably ensued. For this group trials were given in pairs (one positive and one negative); which shape was shown first was varied in a random way from one pair of trials to another. The two trials composing a pair were separated from one another by intervals of 15 minutes, and pairs of trials were separated from one another by intervals of 2 hours. The other two groups of animals (groups II and III) were not presented with a piece of sardine on any positive trial on which they failed to attack within 30 seconds. Their ten daily trials were given at intervals of approximately 50 minutes throughout the day. These training methods will be referred to as Methods A and B respectively.

Scores

Any attack on a positive shape within 30 seconds was counted as a correct response. Any failure to attack a negative shape within 30 seconds was counted as a correct response. Scores are given in terms of the percentage of correct responses to total trials. All statistics were worked on the raw data.

Transfer tests

Some animals were given transfer tests interspersed with days of retraining on the original shapes. On transfer tests no rewards or punishments were given. The transfer test shape remained in the water for 30 seconds, unless it was attacked earlier when it was withdrawn on being attacked. Two methods of giving transfer tests were used. In method A, ten transfer tests were given on any one day of transfer testing, five with a shape expected to be equivalent to the original positive, five with a shape expected to be equivalent to the original negative. Transfer test trials were separated from one another by about 50 minutes. In method B only one transfer test shape was used on any one day, and four trials were given with that interspersed with eight retraining trials on the original shapes, involving reward and punishment. Each trial was separated by about 50 minutes from the next trial.

TABLE I

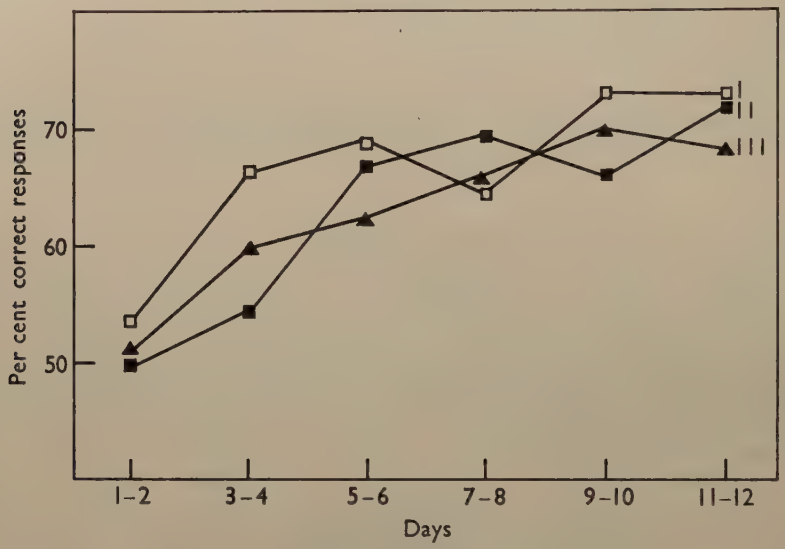
RESULTS OF INITIAL TRAINING: Days 5–12

Group	Animal	Training method	Training shapes +	Training shapes −	Per cent. correct responses: Individual animals	Per cent. correct responses: Group average
I	1	A	○	□	59	70
	2	A	○	□	55	
	3	A	○	◇	66	
	4	A	□	○	82	
	5	A	□	○	70	
	6	A	◇	○	86	
II	7	B	○	◇	73	68
	8	B	□	○	66	
	9	B	◇	○	64	
III	10	B	○	△	76	66
	11	B	○	▽	71	
	12	B	○	◁	61	
	13	B	△	○	60	
	14	B	▽	○	63	

Experimental design

Owing to shortage of tanks, the original experimental design was not completed. Group I (six animals) was trained to discriminate between a circle and a square by training method A; group II (three animals) was trained to discriminate between a circle and a square by training method B; group III (five animals) was trained to discriminate between a circle and an equilateral triangle by training method B. Originally it had been planned to have eight animals in each group; however, the results obtained with the smaller groups are sufficiently consistent to make them worth reporting. The word "square" is to be taken to include both a square with base line horizontal ("normal square") and a square inclined at 45° to the horizontal ("diamond"). For different members of each group different orientations of the square and triangle were used, and different members of each group were trained positively and negatively on the circle. Full details are shown in Table I.

FIGURE 1



Initial learning.

Results

Initial training of groups I and II

Figure 1 shows the learning curves for the three groups. All groups learned to discriminate. It is apparent that at least after the first four days (i.e. the first 40 trials) there is very little difference in the performance of groups I and II. Group I averaged 70 per cent. correct responses, group II 68 per cent. correct over the last eight days (Table I). There is a considerably larger difference in performance over the first four days of training: for these days group I averaged 70 per cent., group II 53 per cent. Although this latter difference is not significant because of the small number of animals in group II, it is in the expected direction. Since group II were not rewarded every positive trial but only on positive trials on which they attacked the shape within 30 seconds the association of reward with the positive shape may well have been less strong for them during the early trials, and this will only be compensated for during later trials when increased hunger increases the tendency to attack. That this was in fact what was happening is indicated by the figures for the total number of attacks on the two shapes combined: group I averaged 5·4 attacks per octopus per day over the first four days, and 4·1 over the last eight days; group II averaged 4·4

attacks over both periods. Thus, making it impossible for the octopus to obtain food unless it attacks the positive shape within 30 seconds seems to lead to fewer attacks during the initial training period (and consequently to a lower level of correct responses), but it helps to maintain the level of attacks over the later part of training. The fact that the poorer initial performance of group II is accompanied by a lower level of attacks means that the difference in performance is not to be explained by the fact that trials are paired for group I and not paired for group II. Pairing of trials should lead to a *lower* level of attacks, since attacks on the second trial of a pair are reduced (Sutherland, 1957a).

Granted that the different methods of training have not influenced scores over days 5–12 of training, we can compare some of the subgroups for these days, making use of the scores of all nine animals in the two groups. Individual animal's scores for these days are set out in Table I. The score for animals trained positively on the circle was 63 per cent., that for animals trained positively on the square 74 per cent.: the difference is not significant ($t = 1{\cdot}8$, $d.f. = 7$, $P > 0{\cdot}1$). There is less difference between the subgroup trained on diamonds and the subgroup trained on normal squares—scores were respectively 72 and 66 per cent.

Transfer tests and further training of groups I and II

(1) *Transfer to different sized figures.* Of group I only the two animals with the highest scores (4 and 6) on the initial discrimination training were used in transfer tests. They were given four days' transfer tests with larger and smaller shapes by method A of transfer testing interspersed with days of retraining on the original shapes. During two days' transfer tests with larger shapes (area 36 sq. cms. as against 25 sq. cms. for the original shapes), they attacked the square (the positive shape) on 17 out of 20 presentations. During two days' transfer tests with smaller shapes (16 sq. cm.) they attacked the square on 19/20 trials, and the circle on 2/20 trials. The levels of correct response for larger and smaller shapes are respectively 90 and 92 per cent., and this is slightly above the level of accuracy which they maintained with the original shapes on the retraining days (86 per cent.), so that complete transfer was evinced. In two days of transfer tests by method A the three animals of group II (nos. 7, 8, 9) scored 77 per cent. correct responses on larger and smaller figures: again transfer occurred without any loss of accuracy.

(2) *Transfer to new orientations.* Animals 4 and 6 of group I were now given two days' transfer tests by method A, with the original circle as one shape, and the square shown in the opposite orientation to the one on which they were trained as the other (i.e. animal 4 was shown a diamond, animal 6 a normal square). Out of 20 trials with each the circle was attacked only twice, the rotated square 11 times. The small number of attacks made on the rotated square is in marked contrast to the number of attacks made on the original square in the intervening day's retraining (19/20), and to the number of attacks made on larger and smaller squares reported in the previous paragraph. χ^2 for the difference between the ratios of attacks to trials with the square for the transfer tests and the day of retraining is 6·5 ($d.f. = 1$, $P < 0{\cdot}02$). Thus the animals were attacking the rotated square significantly less often than the original square. As a further test for transfer to rotated squares, two more days' transfer tests were given in which the circle was shown on half the trials, and over the remaining trials both the rotated and the original square were presented. The circle was attacked 3/20 trials, the original square 8/8 trials, and the rotated square 7/12 trials. Since in both sets of transfer tests the proportion of attacks made on the rotated square is midway between the proportion made on the circle and on the original square, there is no evidence for any transfer from the original square to the

square in its new orientation. From other experiments it is known that where a new figure is shown to which no transfer would be expected, the number of attacks made on it tends to fall about midway between the number made on the original positive figure and on the original negative figure (e.g. transfer from vertical and horizontal rectangles to triangles, Sutherland, 1957a).

In order to see whether there would be any saving in training to discriminate a rotated square from a circle, through having learned to discriminate the original square from a circle, animals 4 and 6 were now retrained to discriminate between a rotated square (positive) and a circle (negative). In six days' training by the method by which they were originally trained, they averaged 68 per cent. correct response; the same animals had averaged 72 per cent. correct response over the first six days of their original training. Retraining was then continued with the other training method, i.e. spaced trials and no reward unless the positive shape was attacked within 30 seconds. Discrimination rose to 87 per cent. correct responses by the 13th and 14th days of retraining. Two days' transfer tests were then given by method B; the animals attacked the rotated square on which they had been retrained on 15/16 trials, the circle on 14/16 trials, and the original square on 16/16 trials, although no reward was given in connection with the original square and they had not been presented with it for the 14 days preceding the transfer tests. It is to be concluded that having learned to discriminate a square in one orientation from a circle, octopuses do not transfer to a square in another orientation, nor does the learning of the discrimination with the square in one orientation facilitate the learning of the discrimination with the square in the opposite orientation. However, the discrimination with the square in its new orientation may be relearned without interfering with the original discrimination.

(3) *Transfer to pentagons.* The two animals of group I (4 and 6) and two animals from group II (8 and 9) were given transfer tests by method B with a regular pentagon of the same area as the squares and circles originally learned. The pentagon was always oriented with its base horizontal. Sixteen trials with each figure were given: the squares were attacked 16 times, the circle 6 times, and the pentagon 8 times. Thus there is no evidence of any transfer from the square to the pentagon. This finding, together with the findings of transfer tests on squares in a new orientation, indicates that the properties of the figures discriminated in the original discrimination were neither the straightness of the sides of the squares as against the continuous curvature of the circle nor the angles of the square as against the lack of angles of the circle.

Initial learning of group III

The learning curve for animals trained to discriminate between circles and triangles is shown in Figure 1, and the scores of individual animals over the last eight days of training are given in Table I. There was no clear-cut difference from the performance of animals trained to discriminate between squares and circles. Animals' scores over the whole 12 days are strictly only comparable with those of group II since group I were trained by a different method. However, since groups I and II did not differ in their scores over the last eight days of training, it seems safe to compare the scores of group III with the scores of groups I and II combined over the last eight days of training. The animals trained on squares and circles averaged 69 per cent. correct responses over these days, whereas the animals trained on triangles and circles averaged 66 per cent. correct responses over these days: the difference is not significant ($t = 0{\cdot}46$, $d.f. = 12$, $P > 0{\cdot}6$). It seems safe to conclude that octopuses do not discriminate more readily between triangles and circles than between squares and circles.

Transfer tests with group III

Two days' transfer tests were given to all animals of group III, using method A of transfer testing. Larger and smaller shapes were used (36 sq. cm. and 16 sq. cm.). Level of response was 62 per cent. accurate on the large shapes, and 72 per cent. accurate with the small shapes. These scores do not differ significantly from the scores on the last day of training and the intervening day of retraining with the original shapes (74 and 62 per cent.). For the transfer to smaller shapes, χ^2 for the difference between the ratios of attacks on positive to trials with positive, and attacks on negative to trials with negative is 9·5 ($d.f. = 1$, $P<0{\cdot}01$), and animals were therefore performing with better than chance accuracy. The ratios for the transfer to larger shapes are not significantly different from one another: however, the difference lies in the expected direction, and performance is within the range found on retraining days, so that it seems safe to assume that transfer was occurring in both cases.

Discussion

Methods

The main findings on methods of training relate to the differences brought about by making reward contingent upon attack on the positive shape within 30 seconds, instead of presenting reward on every positive trial (by showing the sardine with the shape on trials on which an octopus fails to attack the positive within 30 seconds). It was discovered that (1) octopuses could be trained to discriminate by the former method; (2) initial learning is slightly slower, but the same asymptote is reached; (3) fewer total attacks are made in the early stages of training by the former method, but the number of attacks is more steadily maintained throughout training, possibly due to increased hunger. Because in the former method reward and positive shape are less frequently presented together, the association of reward with positive shape would be expected to be less strong in the early trials and this would account for the slower initial learning with the former method. These conclusions are corroborated by Young (1958) who found that where the method of making reward contingent on attack is used animals learn to discriminate between vertical and horizontal rectangles more slowly than, but reach the same asymptote as, animals trained with reward on every positive trial. It has also been found (Sutherland, 1959a) that octopuses trained on a very difficult discrimination by the method of reward every positive trial may learn not to attack either shape until the sardine is inserted with the shape, and as a result discriminate between the two shapes at a very low level of accuracy. With the easier discrimination used in the present experiment, this apparently did not occur since group I improved their discriminatory performance at least up to the 9th day of training. Because it eliminates the possibility of animals learning not to attack until the sardine is presented, and because it saves the experimenter's time, the former method seems preferable and has been used in subsequent experiments.

The properties of shapes discriminated

The most striking finding of the present experiment is that octopuses discriminate between squares and circles as readily as between triangles and circles. This would appear to refute a theory of shape discrimination put forward by Deutsch (1955), at least in its application to the octopus. Boycott and Young (1956) attempted to train two octopuses to discriminate between a circle and a square. One performed at the 59 per cent. level of accuracy during trials 63–118 (corresponding to days 6–12 of the present experiment), the other performed at the 64 per cent. level of accuracy during trials 35–74 (corresponding roughly to days 5–8 of the present experiment). Despite

some differences in the method of training they used, these figures are comparable with the scores attained by the animals of groups I and II in the present experiment.

A further important finding is that octopuses will not transfer from a square with base horizontal to a square tilted to 45° to the horizontal nor *vice versa*. This confirms a finding of Sutherland (1958). This finding agrees with a further finding of Boycott and Young (1956). They trained two octopuses to discriminate between a normal square and a diamond. One animal performed with an accuracy level of 76 per cent. over trials 11–103 (corresponding to days 2–10); the other animal reached an accuracy of 74 per cent. correct response over trials 106–143. This second animal is an atypical octopus in that it only made 14 attacks in the first 105 trials. This suggests that the discrimination of normal square from diamond is easier than that of square from circle, and this is in agreement with the poor transfer found from square to diamond.

The results of the transfer tests enable us to conclude that octopuses were not discriminating in terms of certain properties of the figures. Thus they did not discriminate between the circles and squares in terms of any property which remains constant when the square is rotated through 45°: this eliminates the following properties: (1) Having straight lines *vs.* not having straight lines (cf. also the result with pentagons). (2) Having angles *vs.* not having angles (cf. the result with pentagons). (3) The ratio between the square root of the area and the length of the circumference. (4) The ratio of circumference to longest extent along any one axis. The fact that octopuses gave complete transfer when larger and smaller figures were used eliminates certain further properties of the shapes as forming the basis of discrimination. (5) Absolute differences in amount of circumference. The circumference of the original square was 20 cm. and of the original circle 18 cm., yet the larger circle of circumference 21 cm. was treated as equivalent to the original circle not the original square. A similar line of reasoning indicates that octopuses were not discriminating in terms of (6) the absolute vertical or lateral extents of the figures; (7) the breadth of the shapes across their longest axis.

The present writer has put forward a theory of visual discrimination of orientation and shape in octopus (Sutherland, 1957b). Although the present experiment cannot be regarded as a direct test of the theory, the results of the present experiment appear to fit well with the theory. The theory suggests a possible neural analysing mechanism which operates by dividing shapes into their vertical and horizontal projections and comparing the vertical projections and the horizontal projections of different shapes. Exact predictions for the shapes used in the present experiment would depend upon more precise specification of further analysing mechanisms than is possible at the moment. The present results are consistent with the theory in so far as (1) the theory would not predict any difference in discriminability between triangles and circles, and squares and circles; (2) the theory would predict poor or zero transfer from normal squares to diamonds in so far as the vertical and horizontal projections of these figures are very different from one another (Sutherland, 1957b, 1958). For a direct test of the theory *v.* Sutherland (1959b).

Comparison of results with other data

No previous study has attempted to compare directly the discriminability of squares and circles with the discriminability of triangles and circles for any animal. Evidence on this question must therefore be rather indirect. It is certain that some animals can discriminate between a square and a circle without much difficulty: thus using simultaneous discrimination Coburn (1914) with a crow, and Smith (1934) with cats showed that when these animals have been trained to discriminate between a triangle and a circle, they will transfer to a square and a circle. These results imply

hat crows and cats can readily discriminate a circle from a square. McCallister and Berman (1931) have trained cats to discriminate between circles and squares (with he square in all orientations), and Rowley and Bolles (1935) in a poorly controlled xperiment trained white mice to discriminate between a circle and square to a riterion of 75 per cent. correct responses in 54 trials. It is only the rat (Lashley, 938; Munn, 1931) which has been found to have difficulty with this discrimination, nd because of the rat's tendency to discriminate in terms of the base lines and its bility to discriminate between a diamond and a circle (Lashley, 1938) not very much an be concluded from this. Dodwell (1957) has recently shown that under certain onditions rats can discriminate a square from a circle, and will then transfer the iscrimination to squares and circles of different sizes. In no case has an investigator ested for transfer to rotated squares where animals were originally trained on squares nd circles.

A variety of animals have been trained to discriminate between triangles and ircles: these include chickens (Bingham, 1913, 1922; Katz and Revesz, 1908), the row (Coburn, 1914), rats (Fields, 1932), cats (Smith, 1934), raccoons (Fields, 1936), macaque monkeys (Neet, 1933; Andrew and Harlow, 1945). In the studies by ields, Smith, Neet and Andrew and Harlow good transfer was shown to shapes of different sizes from those on which animals had been trained.

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THE LABILITY OF THE TACTILE-KINAESTHETIC FRAMEWORK

BY

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This paper is concerned with the ability to recognize settings of a rod (pivoted at the centre) at various angles of tilt without the use of vision. Five angles of tilt, all within the upper right quadrant, were used and 80 subjects took part in the experiments. It is shown that tactile-kinaesthetic recognition of a given angle of tilt is decidedly poor, subjects commonly reporting that the position of the rod had been changed. The direction of these subjective shifts was found to be in general towards the centre of the quadrant. If, following the recognition experiments, the subjects were required to judge settings of the rod in fact vertical and horizontal, errors made tended to be in a direction opposite to that normally found in making these judgements. It was further found that attempts to reinstate the original settings led to mean judgements equivalent to those given on the recognition tests, but with signs reversed. These various phenomena are ascribed to lability and disorganization of the tactile-kinaesthetic framework. An analogy with the autokinetic phenomenon is adduced.

Problem and Procedure

We have previously (Sandström, 1952, 1956) reported results with reference to tactile-kinaesthetic perception of verticality with head tilted showing that this perception does not agree with that of visual perception (Aubert phenomenon). The task of the subjects was to move their hands along a rod pivoting at its centre in order to adjust this rod to the apparent upright. A detailed description of the apparatus is to be found in Sandström (1952).

The problems dealt with in this paper are best described by quoting the instructions which were given. Standing beside the experimenter in front of the apparatus, the subject was informed that the experimenter was interested in the following question: How precisely is it possible to judge a given angle of tilt of the rod by moving the hand up and down it in an absolutely dark room? "You have to judge the tilt of the rod as well as you can according to your perception of the tilt. I repeat: I am interested only in your perception of the tilt." Then the experimenter demonstrated the task to be carried out. "The rod in vertical position we call 0°, horizontal position + 90° or − 90° (upper part of the rod is moved to the right and to the left), and the position exactly half-way between is, of course, ± 45°. Here we have + 30° and this position is our starting point for the experiment." (Five different positions were used as starting points.) "Now, please move your preferred hand (only one subject was left-handed) along the rod and try to retain your perception of this tilt. Now I switch off the light and you move your hand again a number of times." After these instructions the light was switched on again and the subject was informed that his task was to judge the tilt of the rod in a series of trials. He was requested to respond as promptly as possible because delay generally gives rise to increased uncertainty in judgement. After asking if the subject wanted more information the experiment began. The apparatus was turned 90° towards the experimenter who, unseen by the subject, moved the rod, but in all the trials in the first experiment the experimenter actually replaced the rod in its original position, thus giving the subject one and the same angle of tilt throughout the whole series of ten trials. The light was switched off and the apparatus turned back in front of the subject. This procedure was repeated after every trial when the experimenter also recorded the judgement given.

The different starting positions of the rod may be called *standards* and the following standards were used: 0°, 30°, 45°, 60° and 90°. Eighty adult male subjects took part in the experiments. All of them were students at the Training College, Fredhaell, Stockholm.

In *Experiment* 1, the subject had to judge the given angle of tilt 10 times, and each subject was presented with only one of the standard positions. The number of subjects was 10 for 0° and 90°, and 20 for 30°, 45° and 60°. This means that we have 100 values for the vertical and horizontal standards and 200 for each of the remaining standards.

Experiment 2 followed immediately upon the conclusion of Experiment 1. The subjects had to judge the position of the rod shifted to either 0° or 90°, the change being made in the same time sequence as before and without any suggestion being given to the subjects. All the 30° subjects and half the 45° subjects had to judge 0°; the other half of the 45° subjects and the 60° subjects had to judge 90°. The 0° and 90° subjects had to judge 90° and 0° respectively. (The results of the last group are not, however, included in Table III because of the special relationship between standards 0° and 90°.)

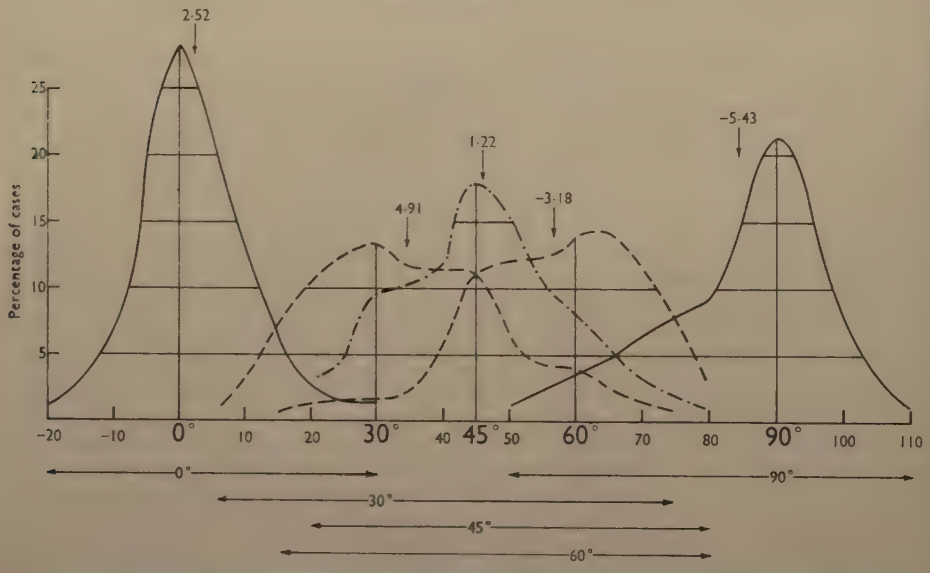
Finally, in *Experiment* 3, the subjects were given the rod adjusted to 0° or 90° and required to adjust it to the standard which they had been given in Experiment 1.

The subjects were asked throughout to stand at a convenient distance from the apparatus and to hold the rod between the thumb and the (slightly bent) forefinger. No special instructions were given regarding posture and no head or body tilt was introduced.

Results

Experiment 1. The smoothed curves in Figure 1 show the distributions of the judgements for the five different standards. The most striking trait is perhaps the wide range of variation, and this range does not vary much with the different positions of the rod. Not even the range with standard 0° is much less than with the other positions; but the greater stability of the vertical is reflected in the definitely lower S.D. (8·27 against 12·29 to 14·22 for the others. The difference between 8·27 and 12·29 is significant: $P < 0{\cdot}001$). Standard 30° shows the widest range (5°–75°); this means that some subjects perceived the rod as if tilted to 5° or 75° in spite of its having been in fact held constant at 30°.

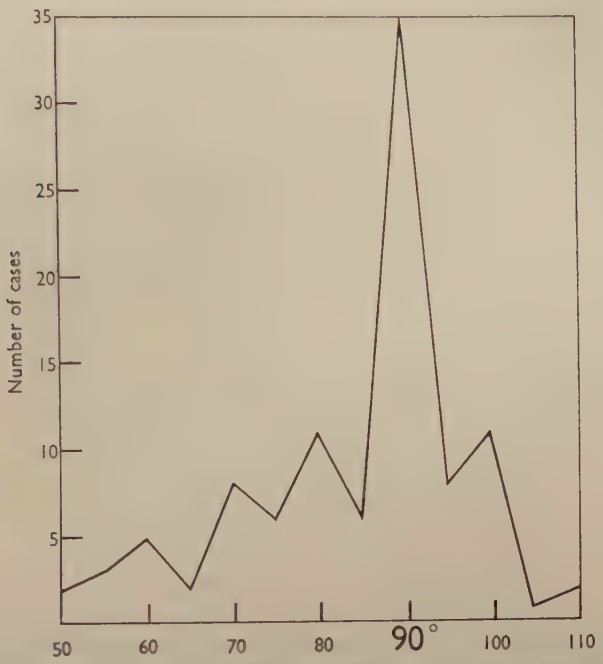
FIGURE 1



Smoothed distribution curves for the different angles of tilt used in Experiment 1. The figures connected to the arrows are the means expressed in deviations from the true positions of the rod (cf. Table 1). The medians are in all cases equal to these positions.

The original curves are characterized by a marked jaggedness which would appear to depend upon the general preference for round numbers. Very few values, and these in the main derived from the same few subjects, are given in figures other than those ending in 0 and 5, and 0 is more common than 5. Figure 2 shows as an example the unsmoothed distribution curve for standard 90°. It will be seen that this preference obtains over practically the whole range.

FIGURE 2



Distributions of raw scores for position 90° in Experiment 1.

TABLE I

RESULTS OF EXPERIMENT 1

	0°	30°	45°	60°	90°	*Total*
N	100	200	200	200	100	800
M	2·52	4.91	1·22	− 3·18	− 5·43	0·15
SD	8·27	14·22	12·93	13·04	12·29	14·06
P	0·05	0·001	—	0·02	0·01	—

Another point which may prove of central interest is the skewness of the curves. As is seen in Figure 1 and in Table I, all the curves tend to skew towards the middle of the quadrant. As a consequence of this, the curve of the standard 45° is the only one not showing any significant mean deviation from its standard. It is perhaps possible to interpret this fact as follows: the main axes in the co-ordinate system so important for our orientation function as a kind of "barrier," e.g. perception has a greater margin to the right of standard 30°, to the left of 60°, and equal on both sides

of standard 45°. But then—why are these margins not equal around the standards 0° and 90°? We do not know the answer, but wish to point out the following:—Using the method of reproduction, subjects with head held upright (as in the present experiment) generally adjust the rod slightly to the left of the true upright independently of the initial position of the rod (Sandström, 1956). Neal (1926) has shown that this holds good also for visual perception. Furthermore, when using subjects with head tilted to the right or to the left, we always get greater deviations to the left of the true upright than to the right (Sandström, 1956). These facts, of course, demand that the rod, in fact upright, must be perceived as tilted in the opposite direction. We know less about the horizontal position, but it may be reasonable to assume that the same perceptual peculiarity is also found for 90°.

Experiment 2 differs from Experiment 1 in that the experimenter introduces a change in the position of the rod. We may postulate that the ten trials of the first experiment have disorganized the tactile-kinaesthetic framework which makes possible more or less accurate judgements of tilt. The assumption underlying Experiment 2 is that this disorganization complicates the judgement of the main axes causing an overestimation of the change. The results given in Table II appear to confirm this hypothesis.

TABLE II

Results of Experiment 2

	N	*M*	*SD*
0°	30	− 1·35	7·65
90°	30	4·35	8·55

If this experiment is performed without previous exposure to the recognition test, the signs are in the opposite directions (Sandström, 1956). Although − 1·35 does *not* deviate significantly from 0°, the difference from Experiment 1 (2·52) shows CR = 2·35, P = 0·02. Further, 4·35 deviates from 90° very near the 5 per cent. level of confidence (CR = 1·93), and the difference from the corresponding value in Experiment 1 (− 5·43) is 9·78, which gives a CR = 4·89 (P < 0·001).

Thus we find here deviations in directions opposite to those in Experiment 1, for standards 0° and 90°. The experiences from the preceding task seem to eliminate the factors which operate when judging these positions under more favourable conditions (cf. the results of Experiment 3 below).

In this trial it is reasonable to assume that guessing has acted as a brake on the hypothesized overestimation. Here, it is easy for the subjects to suppose that the vertical and horizontal positions have been chosen; and this influences the perception. In experiments of this type one must always—as is well known—reckon with various kinds of intellectual considerations in spite of the experimenter's emphasizing that it is "pure" perception in which he is interested. A preliminary trial including Experiments 1 and 2 with ten male and ten female subjects seem to confirm such an assumption. We found no sex differences in Experiment 1—that time including only standard 30°—but in Experiment 2 the negative deviations of female subjects from 0° were significantly greater than those of male subjects. The difference between the sexes is in accordance with other results which, *inter alia*, seem to depend on the female subjects' more spontaneous and immediate attitude as opposed to the male subjects' often more suspicious and reflective attitude.

In *Experiment* 3 the subjects themselves had to readjust the rod from position 0° or 90° to the position of Experiment 1. The instructions were given immediately after

Experiment 2 and were very short: "Now, please, put the rod back to the first position." The question to be answered by this experiment is:—With what degree of accuracy is it possible to reproduce the original stimulus?

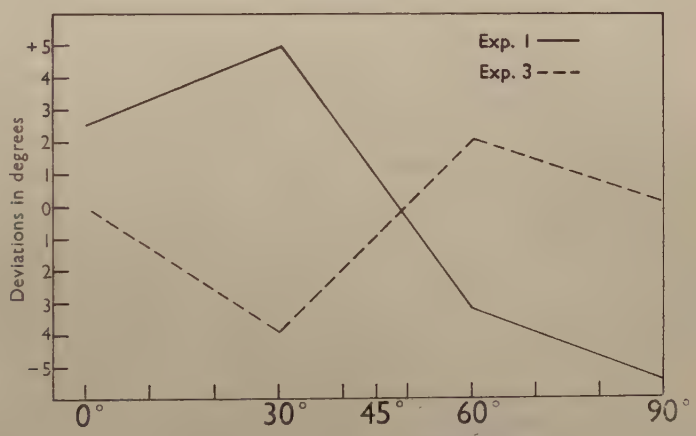
The perfect result of this experiment would be the same figures as in Experiment 1, but with the signs reversed because of the different psychophysical methods used. Actually we get such a result for the three middle standards as is shown in Table 3. In spite of the small N the difference between the values of 30° and 60° shows a good margin of significance at the 2 per cent. level. The mean for all the 80 subjects is − 0·14 against + 0·15 for the 800 measurements of Experiment 1 (Table 1). Figure 3 shows the relationship between the results of Experiment 1 and Experiment 3. Perhaps the perfect rectilinear regression between 30° and 60° over 45° is of some interest.

TABLE III

Results of Experiment 3

	0°	30°	45°	60°	90°	*Total*
N	10	20	20	20	10	80
M	0·10	− 3.90	− 0·85	2·05	0·10	− 0·14
SD	3·00	11·05	8·25	6·90	3·15	7·55

FIGURE 3



Relationships between the results of Experiments 1 and 3.

Discussion

As is to be expected, individual differences are very prominent in experiments of this type. Although these differences are not the main point of this paper, a brief discussion of them is in place.

As illustration of individual differences in Experiment 1, we may give the results of the two subjects showing the highest range of variation. Both fell in the standard 60° group and their values are as follows:—

Subject A: 40° 70° 50° 60° 20° 80° 70° 60° 50° 30°. Range: 60°. Mean = 53°.
Subject B: 45° 70° 30° 25° 35° 25° 25° 25° 15° 15°. Range: 55°. Mean = 31°.

It will be noted that Subject A varied his judgements through the whole series, and indeed his two extreme values 20° and 80°, were consecutive. (Many examples of similar extreme divergences from judgement to judgement also occurred in the data). Subject B, on the other hand, appeared fairly early to stabilize his experience of the position at a very low minus level. It may be remembered that both these subjects were highly intelligent and independently minded, and their judgements may be taken accurately to reflect their perceptions of the tilted rod.

In similar experiments it has been found possible to distinguish two extreme groups of subjects: one judging analytically, rather slowly, and often with an element of suspicion; the other judging naïvely—in the phenomenological sense of the word—and rather quickly. The first type is more commonly found among men, the second among women. In our present Experiment 1, many subjects falling within the first group did not perceive any definite changes in the tilt of the rod during the first trials, but later reported a sudden and distinct change of position. In the second group, deviations were as a rule distributed over the whole series.

A word should be said about possible effects of suggestion in Experiment 1. In the well-known experiments of Sherif (1935) on the autokinetic effect, the instructions were such as to suggest that a definite movement of the spot would occur. In our present experiments, on the other hand, no direct suggestions were given in the instructions. The subjects may, of course, have expected some real changes in the position of the rod—and in some instances they clearly did—but no systematic change in a definite direction or of a definite extent could have been anticipated. It is true that the experimenter's manipulation of the rod between trials had a certain suggestive effect, but his movements were both slight and invisible to the subject. Further, if the subject supposed that the position of the rod had not changed, he was encouraged to report to this effect.

It may therefore be concluded that the subjects did, in fact, perceive ostensible changes in the position of the rod. Although less compelling perhaps than the autokinetic effect, there can be little doubt as to their reality. Some subjects, indeed, spontaneously remarked after a number of trials: "Oh, now it has moved!" but the majority perceived the changes as so self-evident that they said nothing. When asked, they declared frankly that the rod had changed position during the experiment, and that they had reported the extent of these changes. Special trials with very well-trained "phenomenological" observers confirmed this statement.

The various results appear to demonstrate a very considerable lability of the framework upon which tactile-kinaesthetic judgements of position are based. Further studies of this phenomenon in clinical cases or in experimental social contexts (as in Sherif's experiments) might prove of great interest.

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FINGER LOCALIZATION AND FINGER PRAXIS

BY

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The relations between finger localization and various types of finger praxis were assessed by estimates of the inter-correlations among the performance of 100 six-year-old children. Finger localization was related significantly to those types of finger praxis which involve movements of the individual fingers with respect to each other. Right-left discrimination was related to finger localization, but not to the measures of finger praxis.

The findings are interpreted in terms of the construct of the finger schema, which can be viewed as a determinant of performance in both the localization and praxis tasks. This construct can also serve as an explanation of the observed concurrence of finger agnosia and finger apraxia as a consequence of cerebral disease.

Introduction

The relations between "agnosia" and "apraxia" have long claimed the attention of workers in the field of psychoneurology. While the former is designated as a disorder of recognition and the latter a disturbance in the performance of skilled acts, it is often difficult in practice to make a distinction between the two types of behavioural deficit. Since an apractic disorder is by definition a disturbance in the execution of skilled movements within the setting of intact motor functions, it would seem necessarily to implicate impairment of a cognitive or perceptual nature. That such impairment underlies apraxia, especially when the forms known as "ideatory" or "constructional" are under consideration, has been asserted by many observers and a variety of terms (e.g. "agnosia of utilization," "agnosopraxia," "apractognosia") have been coined to express this point of view. Similarly, a basic apratic disturbance has been held to underlie some types of agnosia such as visuospatial agnosia and loss of recognition of body parts.

A type of agnosia in which this question of the contribution of apractic elements has been raised is the particular form known as finger agnosia, i.e. impaired ability to identify fingers either on oral command or when they are tactually stimulated. It has been noted that patients with finger agnosia are often somewhat awkward in the performance of skilled acts involving the use of the fingers and, in some instances, are quite incapable of performing them. Moreover, the somatoperceptual deficit is frequently associated with a form of agraphia which has been described as an "apraxia of writing." This circumstance has led some observers to postulate that finger agnosia occurs as a consequence of a finger apraxia. But this has been denied by others who incline to the view that disturbances of finger praxis, when they occur, are a consequence of the primary agnostic disorder.

Jackson and Zangwill (1952) have attacked this problem of the mutual relations of cognitive and practic factors in finger localization experimentally by studying the execution of discrete finger movements to command in normal subjects under the conditions of direct vision, deprivation of vision and mirrored vision. They found that while deprivation of vision had no differential effect on either reaction time or the incidence of erroneous finger movements, as compared with direct vision, the condition of mirrored vision augmented both types of behavioural deficit. The same deleterious effect of mirrored vision was shown in the performances of patients with

parietal lesions and there was a suggestion that the effect might be of even greater magnitude in such patients than in normal subjects. These findings may be interpreted as showing the very close, perhaps inseparable, association between the "perceptual" and the "practic" factor in performances having to do with localization of the fingers.

In an attempt to contribute further to the elucidation of this question of the relationship between "agnostic" and "apractic" disturbances, the present study investigates the relations between finger localization and a number of measures of finger dexterity in normal children. Finger localization, as defined by identification of fingers subjected to tactual stimulation, shows a progressive development through the age range of 6–9 years (Benton, 1955). Observations on children of superior intelligence indicate that the development of this perceptual skill is a function of mental, as well as chronological age. Further, the fact that many mental defectives make grossly defective performances, as compared with normal children of matched mental age, suggests that integrity of cerebral function is an independent determinant of performance level (Benton, 1955*a*). Similarly, a substantial literature indicates that finger dexterity, as defined by such performances as tapping, manipulation of small objects and differential finger movements, is a function of chronological age. However, when mental deficiency is excluded, these skills appear to show little or no relationship to mental age, independently of chronological age (Goodenough, 1949).

In the present study, the age factor was controlled by restricting consideration to a rather homogeneous group of children in this respect as well as by conventional statistical analysis. In addition, right-left discrimination, a perceptual skill which has been found to be related to finger localization (Benton, 1955*a*), was also assessed.

Method

Subjects and tasks

Subjects were 100 kindergarten children (52 boys, 48 girls) in the public schools of Knoxville, Iowa, whose ages ranged from 5 years, 6 months to 6 years, 5 months. Children in the special kindergarten class for retarded or immature children were not tested. Moreover, setting the upper age limit at 6 years, 5 months insured that no child who, on the basis of age, would have been eligible the previous year for admission to kindergarten was tested. Beyond these limitations, there was no control of the factor of mental age.

The tasks, in the order in which they were given, were as follows:

(1) *Right-left discrimination.* This 16-item task, described in detail elsewhere (Benton and Menefee, 1957), assesses four aspects of the discrimination: (*a*) with the eyes open, identification of single lateral body parts; (*b*) with the eyes open, execution of crossed commands (e.g. right hand on left eye); (*c*) with the eyes closed, identification of single lateral body parts; (*d*) with the eyes closed, execution of crossed commands. Level of performance was defined by the number of correct responses. In previous studies, corrected split-half reliability of the test has been found to be 0·90 or higher.

(2) *Finger localization.* This 50-item task, described in detail elsewhere (Benton, 1955), samples three aspects of the perceptual ability; (*a*) with the aid of vision, localization of single fingers which have been touched; (*b*) without the aid of vision, localization of single fingers which have been touched; (*c*) without the aid of vision, localization of pairs of fingers subjected to simultaneous tactual stimulation. Level of performance was defined by the number of correct localizations. In previous studies, corrected split-half reliability of the test has been found to be 0·90 or higher. Retest reliability, as estimated by the correlation coefficient between scores on equivalent forms administered 20 minutes apart, has been found to be 0·70–0·75. Stability of performance over a short period of time, as estimated by the correlation coefficient between scores on equivalent forms administered 10 weeks apart, has also been found to be 0·70–0·75.

(3) *Finger touching.* Successive touching by the thumb of the fingers from the index finger to the little finger and back again, with double touching of the little finger, was

demonstrated and the subject instructed to execute the series of movements slowly in order to ensure that he understood what was required of him. The subject was then instructed to perform the task as quickly as possible without making mistakes. Two trials with each hand, with the eyes open, were given. This was followed by two trials with each hand, with the eyes closed. Time in seconds, number of errors and type of error (skipping, double touching, reversals) were recorded for each trial. Score was defined as 100 minus (time in seconds for the four trials plus the number of "double touching" and "reversal" errors plus twice the number of "skipping" errors). The errors were differentially weighted to take account of the fact that the "skipping" errors necessarily involved a reduction in performance time. Two scores, one for the "eyes open" trials and one for the "eyes closed" trials, were obtained. Corrected split-half reliability was found to be 0·81 for the "eyes open" task and 0·79 for the "eyes closed" task.

(4) *Paper clips in box.* An open box, 4 in. wide, 2½ in. deep and 1½ in. high, was placed before the subject and 10 (or 20) paper clips spread out evenly in front of it. The subject was instructed to pick up the clips one at a time and place them in the box as quickly as possible. There were two trials with the right hand, two with the left hand and two with both hands. Ten clips were used in the unilateral trials and 20 when the subject used both hands. Score was 120 minus total time in seconds for the six trials. Corrected split-half reliability of the task was found to be 0·89.

(5) *Tapping.* A telegraph key set-up with a platform on which the subject could rest his hand comfortably was used. Tapping with each finger while the other fingers remained stationary was demonstrated. Then each of the subject's fingers was placed over the key with the other fingers spread out on the platform and he was instructed to tap as fast as possible. The task was begun with the right hand (index finger to little finger) and continued with the left hand. Trials were 10 seconds in duration and number of taps for each trial was recorded. Score was the total number of taps for the eight trials. Corrected split-half reliability of the task was found to be 0·83.

(6) *Rey test of differential finger movement* (Rey, 1952). The purpose of this test is to determine whether the subject can make discrete finger movements without the occurrence of associated movements of the other fingers of either hand. He rests his hands lightly, palms down and fingers spread slightly apart, on a sheet of white paper and the contour of the hands and fingers is outlined with pencil. The examiner then places one of his own hands on the table and demonstrates the raising of the thumb and of each of the fingers (about a centimetre) without associated movements of the other fingers. If the examiner himself experiences difficulty in raising the ring-finger without associated movements, the demonstration is terminated before this point and the instructions continued on a verbal level. The subject is then told that he will be instructed to raise one finger after the other, taking care that the other fingers remain immobile. The examiner then points to, but does not touch, the thumb, index finger, etc., of right and left hands. The procedure is repeated, making a total of 20 trials. Associated finger movements are recorded. Score was defined as 25 minus the total number of associated movements in the 20 trials. Corrected split-half reliability of the test was found to be 0·85 in the present sample.

Results

Means and distributions of scores

The means and distributions of the scores for right-left discrimination, finger localization and the five tests of finger dexterity are given in Table I. The obtained mean of 12·4 on the right-left discrimination test is slightly lower than the mean of 13·4 which was secured for six-year-old children in an earlier smaller sample (Swanson and Benton, 1955). The obtained mean of 36·1 on the finger localization test is fairly close to the mean of 34·9 found in the previous smaller sample of six-year-old children and also to the mean of 38·1 found by Wake (1956, 1957) in a relatively large sample of six-year-old Canadian children. Similarly, the median score of 18·7 on the Rey test appears to be quite comparable to the values obtained for French-Swiss children by Rey (1952), who reports a median of 19 for children within the age range 6 years–6 years, 11 months.

TABLE I

MEANS AND DISTRIBUTION OF TEST SCORES

	Mean	*SD*	*Type of distribution*
Finger localization	36·1	6·7	Unimodal, negatively skewed
Finger-touching (eyes open) ..	72·2	7·3	Unimodal, nearly symmetric
Finger-touching (eyes closed)	67·5	8·9	Unimodal, nearly symmetric
Paper clips	30·7	12·8	Unimodal, nearly symmetric
Tapping	146·6	21·7	Unimodal, negatively skewed
Rey test	17·5	4·7	Unimodal, negatively skewed
Right-left discrimination ..	12·4	3·3	J-shaped, negatively skewed

Age and test scores

There was a non-significant product-moment correlation coefficient (0·07) between age and score on the finger localization test. The correlation coefficient (0·10) between age and score on the right-left discrimination test was similarly non-significant. The correlation coefficients between age and scores on the five-finger dexterity tests ranged from 0·21 to — 0·07, the median coefficient being 0·09. In view of these non-significant values, it was deemed unnecessary to partial out the age factor in evaluating the intercorrelations between test scores.

Intercorrelations of test scores

The matrix of product-moment intercorrelations among the several measures of finger localization, finger praxis and right-left discrimination is presented in Table II. In view of the skewness of some of the distributions of scores, bivariate scatter plots corresponding to these correlations were constructed and inspected. The assumption of linearity of regression and homogeneity of variance appeared tenable in every case.

It will be seen that all the intercorrelations are positive in direction. Turning to the question of the relationships between finger localization and the several measures of finger praxis, which is the focus of interest in the present study, it will be noted that there appears to be a substantial difference in the size of the individual correlation coefficients. In the case of three relationships, viz.: finger localization *versus* finger touching (eyes open), finger touching (eyes closed) and the Rey test, the coefficients are of moderate magnitude and clearly significant ($P < 0{\cdot}001$). On the other hand, in the case of the other two relationships, viz.: finger localization *versus* tapping and

TABLE II

INTERCORRELATIONS AMONG TEST SCORES

	Finger local.	*Finger-touch (eyes open)*	*Finger-touch (eyes closed)*	*Paper clips*	*Tapping*	*Rey test*
2. Finger-touch	0·49	—	—	—	—	—
3. Finger-touch (eyes closed)	0·46	0·72	—	—	—	—
4. Paper clips..	0·22	0·34	0·23	—	—	—
5. Tapping	0·18	0·14	0·19	0·21	—	—
6. Rey test	0·45	0·38	0·42	0·21	0·26	—
7. Right-left	0·36	0·08	0·09	0·08	0·04	0·06

paper clips in box, the coefficients are of smaller magnitude and not clearly significant ($P > 0{\cdot}01$). From inspection of the nature of the finger praxis tasks, it will be noted that the higher correlation coefficients appear to be associated with the relationships between finger localization and those tasks which *involve movements of the individual fingers with reference to each other*, while the lower coefficients are characteristic of the relationship between finger localization and the two tasks which do not as clearly involve such "inter-digital" interaction. Moreover, the "inter-digital" motor tasks show a somewhat higher degree of communality among themselves than they do with the paper clips test or speed of tapping. In view of their high correlation ($r = 0{\cdot}72$), finger touching with the eyes open and finger touching with the eyes closed may be regarded as variants of the same task. The mean correlation between these two tasks and the Rey test was 0·40 while the mean correlation between the two tasks and the paper clips test and speed of tapping was 0·28 and 0·16, respectively.

Right-left discrimination was found to relate significantly to finger localization, an observation which is in accord with previous findings. On the other hand, there was no significant relationship between right-left discrimination and any of the motor tasks.

Discussion

The findings clearly indicate a significant correlation between finger localization capacity and certain types of finger praxis. These types of finger praxis (finger touching and the Rey test) have in common the characteristic that they involve movements of the fingers in relation to each other. In contrast, the two motor tasks (paper clips and tapping) which did not relate significantly to finger localization capacity do not as clearly involve finger movements in relation to each other.

Since it was not possible to exercise a precise control over the factor of mental age, the question must be raised as to whether variance in this factor may have been responsible for the matrix of positive intercorrelations among the several tasks. A number of considerations indicate that this is not likely to be the case. As has been pointed out in the Introduction, the available evidence indicates that, when mental deficiency is excluded and the factor of chronological age is controlled, as in the present study, performance on finger dexterity tasks shows no important relationships with mental age; hence lack of control of this factor should not have a significant effect on the size of the observed correlations between finger localization and the motor tasks. Moreover, while both finger localization and right-left discrimination have been found to be related to both mental and chronological age, the latter perceptual skill did not show significant relationships with the motor tasks. Thus the observed significant correlations between finger localization and some of the finger praxis tasks would seem to be a function of the specific nature of the performances.

When one seeks a common factor to account for the observed correlation between finger localization capacity and those motor performances of the fingers which involve movements of individual fingers in relation to other fingers, the concept of the "finger schema" immediately suggests itself. According to Gerstmann (1924), who first elucidated the neurological significance of finger agnosia, this perceptual deficit can be viewed as the resultant of a circumscribed impairment of the body schema, namely, one involving the individual's differentiated concept of the relations of the fingers to one another. Strauss and Werner (1938) designated this partial impairment as a "deficiency in the finger schema." This construct of the finger schema can be applied equally well to those types of finger praxis which involve movements of individual fingers in relation to one another and can serve as an explanation of the observed correlation between finger localization capacity and these

types of finger praxis. It can also account for the clinical observation that finger agnosia and one or another type of finger apraxia frequently occur together as a consequence of cerebral disease.

The fact that right-left discrimination does not relate significantly to these types of finger praxis underscores the specificity of the finger schema construct. On the other hand, a correlation between right-left discrimination and finger localization has been repeatedly observed and a variety of explanations, none quite satisfactory, have been advanced to account for it. Obviously a somewhat broader construct than that of the finger schema is required to account for this relationship.

Thus, from a theoretical standpoint, the behavioural events which are typically designated as finger localization and those which are designated as finger praxis can be considered as reflecting, at least in part, the same (admittedly rather ill-defined) construct of the finger schema. The findings of Jackson and Zangwill, as well as the present findings and the results of clinical observation, support this idea. From this point of view, the controversy in the clinical literature as to whether finger agnosia "causes" finger apraxia, or *vice versa*, seems to be rather fruitless, since both types of behavioural impairment can be conceived as representing a single basic neuropsychological deficit.

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SHORTER ARTICLES AND NOTES

A CASE OF FLUCTUATION OF AWARENESS WITH THE PULSE

BY

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The case is described of a man in whom certain percepts of central origin (after-images of hallucinations) fluctuated rhythmically with the arterial pulse. The phenomenon is explained in terms of contemporary theories of consciousness and the inhibitory effects of baroceptor impulses.

In a previous communication (Oswald, 1957) it was described how some persons may experience visual after-images not only after retinal stimulation but also after the prolonged contemplation of a voluntary vivid image (hallucination). One of those described, Subject Wi, is further discussed below. It is relevant to this case and to the interpretation suggested, to recall that Griffits and Gordon (1924) described the rhythmical increase and decrease of the size, intensity, or apparent distance of vivid images (hallucinations) with rhythmical fluctuations of vasomotor tone, of pulse rate and, by inference, of blood pressure.

Method and results

The general method used for the demonstration of the central after-images involved the subject concentrating on a sheet of blank paper and conjuring up a vivid image. When this appeared, the subject concentrated fixedly upon it for 30–45 seconds, and then looked away at a blank screen. On this screen an after-image of about the size expected by calculation at once, spontaneously and *effortlessly* appeared. In most subjects it would fade away within 5 seconds, but a few perceived such after-images longer. The subject with whom we are at present concerned, was exceptional in being able to perceive it far longer than anyone else (up to 30 or 45 seconds).

At the time of the initial observations, the writer's interest was wholly in the immediate size of these central after-images (which were measured) and little interest was taken in the fact that this one person alone remarked spontaneously that, after about 5 seconds, his after-image began to pulsate, going rapidly in and out and thus gradually shrinking smaller and smaller till it disappeared. This pulsation was not present in the primary image and was only faintly perceptible in the after-images of real visual patterns.

About a week after carrying out these measurements of after-images with the subject, it occurred to the writer to question him whether the pulsation was very rapid. When the subject indicated the approximate rate, the writer explained that he had wondered if it was much faster, if in fact it flickered at about 10 per second, the alpha rhythm rate. Lindsley (1952) has reviewed the evidence for fluctuation of consciousness at the rate of the alpha rhythm. The subject explained that it was much slower than that, remarking that, if anything, he supposed it would be about the rate of his heart.

Some five months later it was decided to investigate the pulsation of the central after-images further.

The subject was seated in a chair with electrodes on his head, a further electrode on the left arm (the casual remark being made to the effect that this was to measure changes in muscle tension in the arm), for recording the electrocardiogram. A further electrode was fixed to the wooden arm of the right side of the chair. It had previously been shown that tapping on the arm of the chair would, by this means, give a recordable signal.

The subject was in one room, while the electroencephalograph was in another. The rooms were separated by a corridor and two closed doors. The clicking of the electroencephalograph, each second, was inaudible in the room in which the subject sat.

He was told that it was intended to study the electroencephalographic and muscle tension changes occurring with his after-image. He was asked to try to tap in time with the inward phase of the pulsation. At the end of the session he was deliberately asked if

he had thought that his heart rate was being recorded. He replied that he had supposed the spring earth clip on his ear to serve for recording his pulse.

Five recordings were made in which he tapped in time with the ingoing phase of his after-image. Four of these, of 31 beats, 22 beats, 17 beats and 11 beats respectively, showed correspondence between his rate of tapping and his heart rate (*see* Figs. 1 and 2). The other showed correspondence for the first 10 of 30 beats.

FIGURE 1

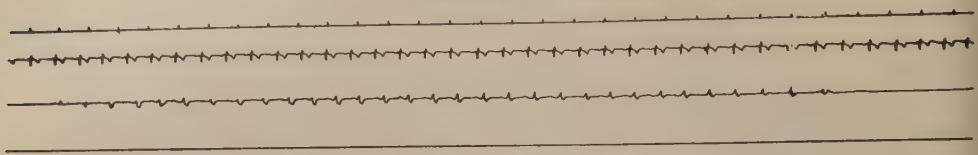
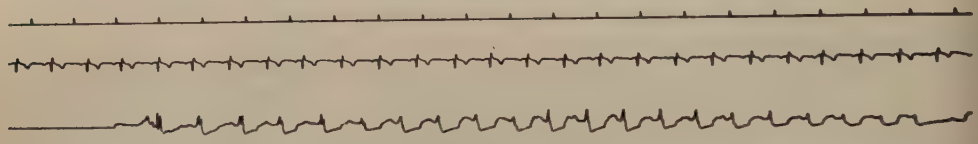


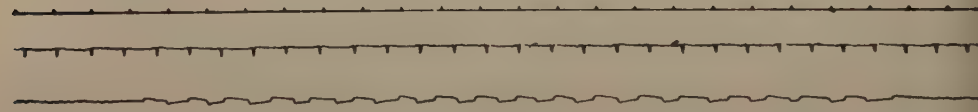
FIGURE 2



Showing, from above downwards, time-marker (seconds), electrocardiogram, and hand movements of subject in time with rhythmic decrease in size of his after-image

The subject was closely questioned in regard to whether he might be able to feel his heart beating, or feel or hear pulsations anywhere in his body, and, if so, whether he was aware of his after-images beating in time with them. He denied this. Further experiments were carried out by the writer and an assistant in which both tried to tap in time with their heart rates. None of these records showed any sort of correspondence between the rhythms, and indeed, it is a most difficult task. One may believe that one can feel faint beating sensations, but as soon as the arm starts tapping, attention becomes diverted to the much stronger sensations of the tapping, and any awareness of the heart or pulse is lost. Two similar recordings were made with the subject, who was asked to close his eyes and to try to tap in time with his pulse rate. Neither record showed any correspondence between the two rhythms (*see* Fig. 3). He affirmed that he had really tried, and concen-

FIGURE 3



Showing, from above downwards, time-marker (seconds) electrocardiogram, and hand movements as subject attempts to tap in time with his pulse. Note decreased decisiveness of movement

trated on feelings in his chest and eyeballs and he seemed to believe he had probably been tapping in time with these sensations, though agreed it was very difficult. Figure 3 indicates the decreased confidence of his tapping in the absense of a visual guide.

Discussion

It must be stated that there were no grounds for believing this man to be deliberately lying in saying that he was unaware of his pulse when sitting in the chair. He several times mentioned the after-image pulsation quite spontaneously, before the time when the writer showed interest in it.

Several hypotheses present themselves. One is believed to merit particular consideration—that the neuronal correlate of the after-image in the fore-brain persisted and that it was rhythmically "activated" by ascending influences from the reticular formation of

mid- and hind-brain. This accepts the contemporary belief that consciousness depends upon the interaction of, for instance, specific cortical effects of external stimulation and facilitative impulses ascending from the reticular formation.

It is known that with each pulse a sharp afferent volley of action potentials ascends from the baroceptors of the carotid sinuses (Bronk and Stella, 1932). These impulses reach the medullary and mesencephalic reticular formation and thence cause cortical inhibition (Bonvallet, Dell and Hiebel, 1954; Nakao, Ballin and Gellhorn, 1956). Increased carotid sinus pressure causes sleep-like states, not only in persons with hypersensitive carotid sinuses (Ferris, Capps and Weiss, 1935) with the production of electroencephalographic slow waves (Engel, Romano and McLin, 1944), but also in normal men (Schlager and Meier, 1947) and dogs (Koch, 1932). The effect can be independent of reflex bradycardia or hypotension.

Some support for the view advocated is offered by the following facts. Bonvallet, Dell and Hiebel (1954) described the rhythmic fluctuation of the level of cortical excitation with the rhythmic fluctuation of blood pressure (Mayer waves). These waves, often wrongly called Traube-Hering waves (*see* Schweitzer, 1945), occur in man. Intra-arterial blood pressure records in man have shown that in many persons there occur, as would be expected (Gunyton and Harris, 1951), oscillations of this homeostatic system about the mean, of the order of 20 mm. of mercury about every 10 seconds. In at least some circumstances these oscillations may synchronize with fluctuations of heart rate and skin vasomotor tone (e.g. Matthes, 1952). Roy and Sherrington (1890) suggested that this rhythm was responsible for "the rhythmic variations in the acuity of auditory and visual perception." Their suggestion was confirmed by several workers (e.g. Griffits and Gordon, 1924).

It is important to recall that a whole host of different internal and external influences summate to govern the level of activity of the ascending, activating reticular formation (and that the descending impulses arising during voluntary attention are probably of major importance). Such other factors can easily mask any effects of carotid-sinus impulses, as Bonvallet, Dell and Hiebel (1954) were at pains to emphasize. Therefore the spontaneous, *effortless* and wholly *central* after-images of the subject described would seem particularly open to the effects of carotid-sinus impulses. External stimuli can, of course, be responsible for both vasomotor changes and changes of cortical excitation, as Ackner and Pampiglione (1957) have described, but as these authors pointed out, so can internal stimuli and among these, impulses from the carotid sinuses merit consideration.

The observations described were carried out while the writer was serving in the Electroencephalography Department, Princess Mary's Royal Air Force Hospital, Halton, Buckinghamshire and during the period of tenure of a British Medical Association Research Scholarship. It is a pleasure to acknowledge the encouragement of Group Captain V. H. Tompkins, Principal Specialist in Neuropsychiatry, Royal Air Force.

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NOTE ON LUMINOUS FLUX DISCRIMINATION IN MONKEY AND MAN

BY

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Subjects were instructed to equate the brightness of two stimuli of different areas presented alternately to the far periphery. The results are closely describable by $IA = k$ (Ricco's Law), indicating that the subjects equated total luminous flux. It is suggested that these results and certain other behavioural and anatomical considerations make it unnecessary to consider flux discrimination a "subcortical" or "lower" visual function as compared with brightness discrimination.

Klüver (1936, 1941, 1942), studying the visual functions of rhesus monkeys after removal of the occipital lobes, concluded, *inter alia*, that "the bilateral occipital monkey cannot respond to differences in brightness but only to differences in the density of luminous flux entering the eye." This finding has been cited in support of theories of the phylogeny of visual function (Fulton, 1949) and of the role of the striate cortex (Morgan, 1951; Klüver, 1942, 1951). Klüver, however, has not published any anatomical studies of the extent of the lobectomy. In as much as the cortical tissue mediating peripheral vision extends quite far forward on the medial surface, the possibility exists that Klüver did not remove all of this tissue and thus his animals may have retained some peripheral vision (cf. Fig. 1). Since the structure of the

FIGURE 1.

Medial aspect of the brain of *Macaca mulatta*, after Brodman. The extent of the striate cortex is shown by stippling. Redrawn from Marquis and Hilgard (1937).

human retina appears to be very similar to that of the monkey retina (Polyak, 1941) and since the visual capacities of the far human retina are similar to those Klüver reported for his monkeys, i.e. both can discriminate the brightness of areas of the same size and neither can discriminate pattern, we undertook to examine the above possibility by studying the supra-liminal area-intensity relationship in the far human periphery. Although the area-intensity problem has aroused renewed interest in recent years (e.g. Blackwell, 1946; Willmer, 1950; Riopelle and Chow, 1953; Gregory, 1956; Barlow, 1958), no studies of peripheral supra-liminal relationships have appeared which bear directly on the problem at hand.

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Method

Apparatus. Light from a 12-watt bulb was directed through projector lenses and two polaroid filters to fall upon a white screen. The intensity of the light was controlled by varying the angle of one of the polaroids and was calibrated by means of a Salford photometer. The projected stimuli were circular and their sizes were controlled by an opaque slide containing holes of different sizes inserted between the light source and the lens system. Two such optical systems were located equidistant from the screen enabling two stimuli of different sizes and intensities to be presented in alternation at the same position on the screen. The centres of the stimuli were projected 55° to the right of a small red fixation light. Both the fixation point and the stimuli were located 81 in. from the subject and at his eye level. The sizes and intensities of stimuli employed are shown in Table I. Since Klüver did not state the actual range of stimulus sizes he used, we selected our sizes to avoid a clearly perceptible difference of size between members of each pair of stimuli.

Procedure. The subjects were seated in a darkened room; their left eyes were covered. The following method of adjustment was employed: the stimulus from each lamp was presented alternately at the same position on the screen for periods of approximately 2 seconds. The size of both was controlled by the experimenter. The intensity of one stimulus (I_b) was set by the experimenter and the subject was asked to adjust the intensity of the second stimulus (I_a) until both stimuli appeared equally bright. For any given I_b three determinations of I_a were made, one with the initial intensity of the adjustable stimulus (I_a) greater than that of the constant stimulus (I_b), one with it equal to the constant stimulus and one with it less than the constant stimulus. The stimuli were alternated until the subject was satisfied that he had adjusted I_a to equal I_b. The process of equating the two stimuli never took longer than 1 minute. Immediately after each such adjustment the lights in the room were turned on. Each subject was given five practice runs before each experimental session in addition to a short training session before the experiment was begun. The subjects were never given instructions on the meaning of "brightness." (In a preliminary training session when viewing stimuli of different sizes centrally, having been instructed to use "brightness" as the criterion of equality, they always equated the stimuli on the basis of their physical intensity. See Fig. 2*a*). Two of the subjects, R. J. S. and M. F. T., were not told the purpose of the experiment and were not aware of the actual relationship of the stimuli. One of the authors (L. W.) served as the third subject.

Results

When the subjects were asked to equate the brightness of stimuli of different areas which were received by the far periphery of their retina they tended to equate the total luminous flux of the stimuli. That is, the subject adjusted the intensity of the variable stimulus (I_a) so that its luminous flux ($I_a \times A_a$) closely approximated the flux of the fixed stimulus ($I_b \times A_b$). This is shown in column 5 of Table I, where

TABLE I

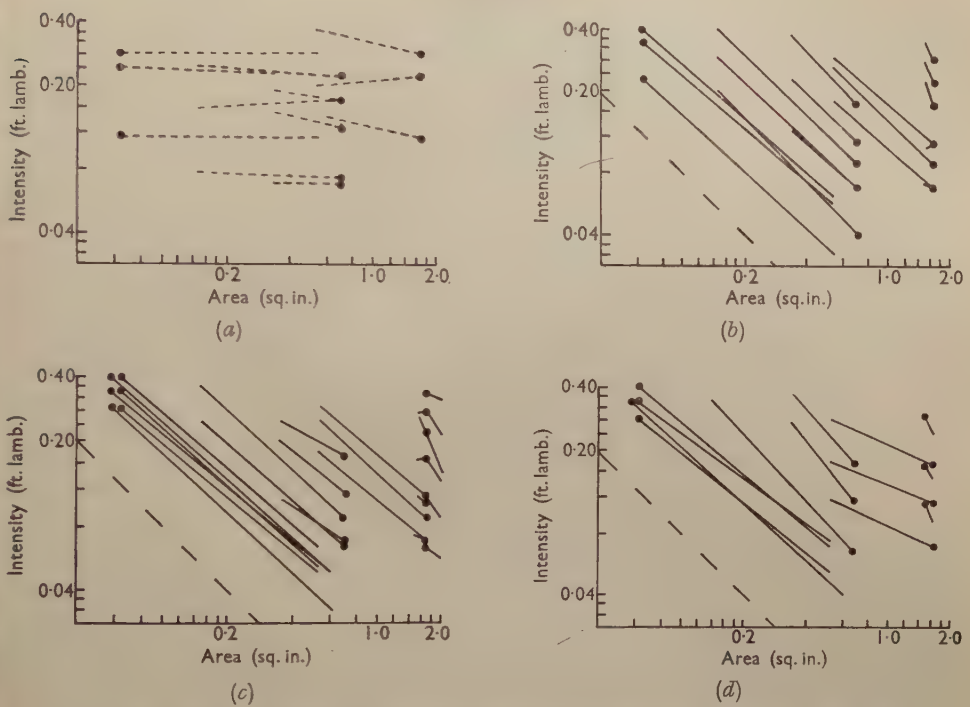
Intensities and Areas of Stimulus Pairs

1. *Angular diameter (degrees)*		2. *Area (sq. in.)*		3. A_b/A_a	4. *Range* I_b *(ft. lamb.)*	5. *Average flux ratios* $(I_a \times A_a/I_b \times A_b)$		
D_a	D_b	A_a	A_b			*R. J. S.*	*M. F. T.*	*L. W.*
2·0	0·6	6·4	0·6	0·09	0·22–0·40	—	1·14	1·05
1·9	0·6	5·7	0·7	0·12	0·11–0·40	1·04	1·16	1·26
3·6	3·3	20·7	17·4	0·84	0·07–0·34	—	0·98	—
3·2	3·3	15·8	17·4	1·1	0·07–0·31	1·05	0·96	0·83
1·6	2·1	3·6	7·2	2·0	0·07–0·18	1·00	0·82	1·00
2·0	3·3	5·7	17·4	3·1	0·07–0·11	0·99	0·93	1·64
1·0	2·1	1·5	7·2	4·8	0·07–0·08	1·02	0·87	1·03

(The subject was free to vary I_a from 0·03 to 0·42 foot lamberts.)

the ratios of the flux of the variable stimuli to the flux of the fixed stimuli ($I_a \times A_a/I_b \times A_b$) may be seen to approximate unity. In the case of central fixation, of course, this ratio approximates the ratio of the areas (A_a/A_b). In Figure 2 the matching of stimuli by each subject for each value of the fixed stimulus is graphed. Area of the stimulus is plotted against intensity of the stimulus, both on a log scale. Each line connects the value of a fixed stimulus (black circles) with the value of the variable stimulus (no circles) equated to it by the subject. Figure 2*a* represents central fixation and Figures 2*b*, 2*c* and 2*d* far peripheral fixation.

FIGURE 2



Area of the stimuli plotted against intensity of the stimuli, both on a log scale. Each line connects the value of a fixed stimulus (black circles) with the value of the variable stimulus (no circles) equated to it by the subject. The dashed line represents IA = *k* or Ricco's Law. (*a*) Central fixation, R. J. S. (*b*) Peripheral fixation, R. J. S. (*c*) Peripheral fixation, M. F. T. (*d*) Peripheral fixation, L. W.

The relationship found to hold approximately in the far periphery may be formulated as IA = *k*. This theoretical relationship can be represented as a 45 deg. line (dashed line in Figures 2*b*, 2*c* and 2*d*). It has been found to hold within a limited size range for the absolute and differential threshold and in these cases is known as Ricco's Law.

The present study can hardly be considered a systematic investigation of the limits within which the flux relationship holds in the far periphery. All the same, it is of interest to note that the stimulus area for which the relationships holds is of a somewhat larger order of magnitude than that found for the differential intensity threshold. Thus, Bouman (1950) reported that Ricco's Law held only for areas smaller than 15 min. at 7 deg. from the fovea. Brindley (1954), using stimuli of different colours at 55 min. and 3 deg. from the fovea, found that Ricco's Law held for violet on green or red for areas smaller than 4 min. and 5 min. at retinal position of 55 min. and 3 deg. respectively. In the present study the

flux relationship held for stimuli of the order of at least 2 deg. in diameter. (Note that when the ratios are not near unity but involve large areas, the results for one subject (L. W.) tend to deviate from the predicted flux relationship toward the intensity relationship, suggesting that we were starting to depart from Ricco's Law with stimuli in the region of 3 deg.) This difference between our results and the earlier results may be due to the fact that we were measuring equal brightness matches rather than the differential threshold. Or it may be due to the greater optical distortion in the far periphery. Equally it may be due to the greater convergence of rods on bipolar cells in the far periphery. However, this generally accepted view of a gradient of convergence across the retina (Morgan and Stellar, 1950; Polyak, 1939) has been challenged (Vilter, 1949).

Discussion

Our experimental situation appears to be very similar to that in which Klüver placed his bilateral occipital monkeys. In both cases the subjects had been trained to discriminate the brightness of two stimuli of equal area and both lacked "pattern" discrimination. When presented with a pair of stimuli of different areas both approximately equated the total luminous flux of these stimuli. We wish to suggest the possibility that these experiments are not merely analogous but homologous; that Klüver's postoperative monkeys may have retained some of their far peripheral vision. Giving no anatomical reconstructions, Klüver merely reported "extirpation of the occipital lobes" and that "as indicated by microscopic examination the striate cortex had been completely removed on both sides." (1941.) The striate cortex of the macaque, after sinking into the depths of the calcarine fissure, "extends forward almost to the splenium of the corpus callosum, well beyond the level of the medial parietooccipital fissure." (von Bonin and Bailey, 1947). As Figure 1 shows, "occipital lobectomy" would not necessarily remove the most anteromedial regions of the striate cortex. In fact when Polyak (1933) removed the left occipital lobe of a macaque he found that the anterior third of the striate area in the calcarine fissure was preserved. As a result the lateroventral and medioventral segments and the ventral anterior half of the intermediate segment of the ipsolateral lateral geniculate body did not degenerate. As he points out, Brouwer and Zeeman (1926) have shown that the portions spared are mainly those where the most peripheral regions of the retina terminate. (In a second case Polyak did remove all the striate area.) Similarly, Walker and Fulton (1938) found some anterior tissue remaining after occipital lobectomy in the chimpanzee.

Furthermore, Klüver reported that the vision of his operated animals was greatly impaired under daylight conditions as compared with dark adaptation conditions, suggesting the presence of some peripheral vision. In view of these anatomical and behavioural considerations the hypothesis that Klüver's animals may have retained some tissue which mediated peripheral vision seems to us entirely reasonable.

On the other hand, we do not wish necessarily to support the view that complete striate removal would *not* produce "flux" results in the monkey. Marquis and Hilgard (1937) were able to produce conditioned eyelid reponses to light in monkeys without their occipital lobes, and in which there was apparently complete degeneration in the lateral geniculate bodies. But, from their evidence, it is not possible to say whether such monkeys responded to total light energy (flux), as such, or to intensity (energy/area). Lashley (1931) reported that in rats completely deprived of their striate cortex size discrimination broke down when the fluxes were equated. On the basis of comparing the effects of striate cortex removal in cat and guinea-pig on the residual optokinetic response, Smith (1937) comments that "the nature of this residual capacity seems to be similar in every respect to that found in animals possessing only rods in the retina." Our investigation, therefore, is relevant to the

question of whether extreme peripheral, or rod, vision characteristically yields a discrimination based only on flux. Our results would seem to contradict the assertion by Lashley that "this (flux discrimination) is not characteristic of human peripheral vision" (1931), but he cites no supporting evidence. It is clear that this question is quite independent of the more specific question of whether Klüver's monkeys did or did not have any residual striate cortex following occipital lobectomy.

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CHANGES IN THE SIZE AND SHAPE OF VISUAL AFTER-IMAGES OBSERVED IN COMPLETE DARKNESS DURING CHANGES OF POSITION IN SPACE

BY

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It is well known that if a visual after-image is "projected" upon a wall, or screen, in a semi-darkened room, the apparent size of the after-image is a function of the distance of the surface upon which it is "projected." The size increases with distance, almost in direct proportion to this distance. This is known as Emmert's Law (Emmert, 1881). It is of considerable interest, for it is a convincing demonstration that apparent size is not a simple function of the size of the retinal image, and that many perceptual factors influence the apparent sizes, and distances, of objects. The history of this idea, together with the classical experiments, are described by E. G. Boring (1942).

In carrying out some experiments on after-images produced by a short bright flash of light provided by a 110 joule 1 m.sec. flash tube, a number of curious effects have been observed which would seem to be related to Emmert's effect. It is important to stress that the effects to be described were observed in complete darkness, after the flash, and thus differ in an essential respect from the conditions for Emmert's effect. Since the flash duration was only 1 m.sec. no appreciable movement of the eyeball could occur in the stimulus period. This technique gives after-images of great clarity and detail, which is essential for these new effects.

1. When the head is moved, even by a few centimetres, forward or backwards, the after-image changes in size. It increases in size as the head is moved back, and decreases as it is moved forward. This effect is most marked, and may be observed after fixating an ordinary electric light bulb for a few seconds provided the after-image is sufficiently distinct. Ten of the subjects were experienced research workers in the psycho-physiology of vision. In addition, the effect has been demonstrated in some 40 relatively naïve subjects, mainly undergraduates. So far, all subjects have observed this effect.

2. A similar effect may be observed with the head stationary. The subject "projects" the after-image of the flash tube on to his outstretched hand, still in complete darkness, and slowly moves his hand to and from his eyes. One of two effects may be observed. (*a*) The after-image may seem to remain on the hand, in which case it shrinks as the hand recedes and expands as it approaches. (*b*) the after-image may seem to remain fixed in space and to remain the same size. If the proprioceptive locus of the hand lies between the after-image and observer, the after-image may wholly or partially disappear, as though occluded by an opaque object.

Whether (*a*) or (*b*) occur evidently depends on the "set" of the observer. Of the ten trained observers, three have reported the "occlusion" effect on some occasions. All have observed the size change under these conditions.

3. When the observer's hand, or a screen held in his hand, is kept at constant distance, say at arm's-length, and the observer moves his head and shoulders and the hand or screen back and forth, then the after-image remains of constant size. It is perceived as an object situated at constant distance.

4. If the flash tube is directed on to the subject's hand, so that an after-image of the hand is produced, some curious phenomena may be observed, but these are difficult to describe briefly. The visual and proprioceptive loci of the hand may separate in a disconcerting manner. Further complex effects occur if the subject attempts to pick up an object viewed as an after-image.

5. When the observer changes his position in space, perspective changes may take place in the after-image. For example, an after-image may be obtained of a view down a long corridor. With the flash technique this will have unusual clarity. If the observer then walks across the corridor, looking down it, as it were, then his after-image may change in perspective as he moves. These perspective changes are not easy to assess, however, for they vary from time to time.

To observe these effects, it is most important that the eyes be steadily fixated while the after-image is being observed. The short flash technique for producing the after-image ensures that movement of the retina is unimportant during the production of the after-image, which is unusually detailed, but large eye movements after the initial flash temporarily disrupt the after-image and cause it to fade rapidly. The reason for this is not known.

We thought it important to discover whether at least the main effect—the change in size of the after-image with change in position of the observer—is directly related to acceleration forces applied to the observer. To test this, we have examined the effect under conditions of maintained angular acceleration in the Farnborough human centrifuge. Angular accelerations up to 3 g. were used. This was arranged through the kindness of Squadron-Leader T. C. D. Whiteside who, with Professor R. C. Oldfield, R.L.G. and F.W.C., acted as observers. The sensation of movement in the centrifuge is marked during deceleration, when the well-known "tumbling" sensation occurs, the observer apparently falling forward head over heels. In this situation the after-image of the flash tube may expand, as though the observer is falling into it. It seemed clear to all four observers that the magnitude of the effect is not a simple function of "g," but may be related to the subject's impression of his movement, or the movement of his head, in space.

As a speculation, we may suggest that at least some of these phenomena, and also Emmert's original observation of after-images "projected" on to a visible screen, may be related to Size Constancy. When an object is viewed normally, decrease in the observer's distance from an object will produce a corresponding increase in the size of the retinal image, but the perceived size of the object may remain almost constant. In the special case of the after-image the image on the retina does not change size as the observer changes his position in space. If the compensation system which normally maintains perceptual size almost constant when the retinal image changes in size were functional in the after-image situation, then we should expect the kind of effects reported here when the observer changes his position in space. If this interpretation is correct, it gives us a technique for studying the Constancies in dynamic situations.

We are most grateful to Squadron-Leader T. C. D. Whiteside and Professor R. C. Oldfield for constructive advice and active help with the observations.

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ATTENTION IN DICHOTIC LISTENING: AFFECTIVE CUES AND THE INFLUENCE OF INSTRUCTIONS

BY

NEVILLE MORAY

From the Institute of Experimental Psychology, Oxford

In shadowing one of two simultaneous messages presented dichotically, subjects are unable to report any of the content of the rejected message. Even if the rejected message consists of a short list of simple words repeated many times, a recognition test fails to reveal any trace of the list. If numbers are interpolated in prose passages presented for dichotic shadowing, no more are recalled from the rejected messages if the instructions are specifically to remember numbers than if the instructions are general: a specific set for numbers will not break through the attentional barrier set up in this task. The only stimulus so far found that will break through this barrier is the subject's own name. It is probably only material "important" to the subject that will break through the barrier.

Introduction

Cherry (1953) introduced the method of "shadowing" one of two dichotic messages for the study of attention in listening, and found that subjects who shadowed a message presented to one ear were ignorant of the content of a message simultaneously presented to the other ear. By "shadowing" is meant that the subject, while listening to a continuous message, repeats it out loud at the same time. The first experiment was to test more rigorously Cherry's findings.

Method

In all the experiments the apparatus used was a Brenell Mark IV stereophonic tape-recorder modified with twin amplifiers to give two independent outputs through attenuators, one output going to each of the earpieces of a pair of headphones. Matching for loudness was approximate, by asking the subjects to say when two messages that seemed equally loud to the experimenter were subjectively equal to them. The proceedure used allows matching to within $\pm$ 1 db., a difference which has been found (Moray, 1958), to cause no significant favouring of the louder message. The subjects were undergraduates and research workers of both sexes. Before each experiment the subjects were given four passages of prose to shadow for practice. In all cases the loudness of each message was approximately 60 db. above the threshold of the subject, and the speech rate was about 150 words a minute. All passages were recorded by one male speaker.

Experiment I

A short list of simple words was repeatedly presented to one ear of the subject while he shadowed a prose message presented to the other ear. The word list was faded in after shadowing had begun, and was equal in intensity to the shadowed message. At the end of the prose passage it was faded out so as to become inaudible as the prose finished. The word list was repeated 35 times. After asking the subject to report all he could of the content of the rejected message he was given a recognition test using similar material, present in neither the list nor the passage, as a control. The gap between the end of shadowing and the beginning of the recognition test was about 30 sec. The results are shown in Table I.

TABLE I

RECOGNITION SCORES FOR WORDS FROM SHADOWED AND REJECTED MESSAGES

	Mean number of words recognized
Words presented in shadowed message	4·9 out of 7
,, ,, rejected ,, ...	1·9 out of 7
Words presented for the first time in recognition test ..	2·6 out of 7

There is no trace of material from the rejected message being recognized. The difference, however, between the new material and that from the shadowed message is significant at the 1 per cent. level. The 30-sec. delay is not likely to have caused the rejected material to be lost since words from early in the shadowed message were recognized, and the work of Ward (1937) would suggest that this pause might actually increase the score. The present findings are in agreement with those of Cherry (1953).

Although the content of the rejected message seems to be blocked gross changes can be recognized (Cherry, 1953), and there is much anecdotal evidence to suggest that in selective listening the block can be broken down by certain sorts of material. Mothers hear children crying, and it is always said that a subject will respond to his own name even though the signal/noise ration is low, as at a cocktail party (to give an everyday situation). It was decided to investigate this experimentally to find the limits of the efficiency of the attentional block.

Experiment II

Subjects were required to shadow ten short passages of light fiction. They were told that their responses would be recorded and that the object of the experiment was for them to try to score as few mistakes as possible. In some of the passages instructions were interpolated, but except in two of these cases the subjects were not warned of these. In half of the cases with instructions these were prefixed by the subject's own name. The order of presentation is shown in Table II.

TABLE II

Passage	*Instructions at start of passage*	*Instructions within passage*
I	Listen to your right ear	All right, you may stop now.
II	,, ,, ,, ,, ,,	No instructions.
III	,, ,, ,, ,, ,,	John Smith, you may stop now.
IV	,, ,, ,, ,, ,,	No instructions.
V	,, ,, ,, ,, ,,	Change to your other ear.
VI	,, ,, ,, ,, ,,	No instruction.
VII	,, ,, ,, ,, ,,	John Smith, change to your other ear.
VIII	Listen to your right ear: you will receive instructions to change ears.	Change to your other ear.
IX	Listen to your right ear.	No instructions.
X	Listen to your right ear: you will receive instructions to change ears.	John Smith, change to your other ear.

The "no instructions" passages were interpolated in the table at random. For any pair of passages with instructions that without the name was given before that with the name, as it was thought that it was less likely that the former would be heard, and hence less likely that a "set" for that particular instruction would develop before

its second presentation. Most of the subjects ignored even the instructions that were presented in the passages that they were shadowing, and said that they thought that this was just an attempt by the experimenter to distract them. The passages were read in a steady monotone at about 130 words a minute by a single male voice. They were checked with an Avometer to ensure that there was no significant increase in intensity when the subject's name was spoken. The variation was within the limits suggested by Moray (1958). The subject's responses were tape-recorded and later analysed. The subjects were students and research workers.

The results are given in Table III.

TABLE III

Relative Frequencies of Hearing of Affective and Non-Affective Instructions when Presented in the Rejected Message

	Affective (instructions preceded by by name)	*Non-affective (instructions not preceded by name)*
Number of times presented ..	39	36
Number of times heard ..	20	4

(pooled data for 12 subjects)

Since there were 12 subjects, and the data are pooled, there should have been 36 sets of instructions preceded by the subject's name presented in the rejected message. The discrepancy is due to three subjects who heard the instructions and actually changed over, so that the second set of instructions which would normally have been heard as part of the shadowed message were now heard as part of the rejected message. All these cases occurred in passage X. The mean number of instructions heard when presented in the rejected message was calculated, and the difference between the "names" and "no-names" situation submitted to a t test. The difference is highly significant, $t = 3{\cdot}05$, giving a level of confidence greater than the 1 per cent. level, where $t = 2{\cdot}81$. This suggests that the affective value of a message is very important in determining whether it will break through the attentional barrier in dichotic selective listening.

On only four out of the 20 occasions on which the "names" instructions were heard did the subjects actually make a change to the other message, and in one of these cases the subject spontaneously reverted to the first message without noticing. On the other occasions the subjects reported hearing the instructions when asked for introspections in between successive passages or at the end of the experiment. The relative frequencies of hearing with the different instructions are given in Table IV.

TABLE IV

Instructions	*Mean frequency of hearing instructions in rejected message for 12 subjects*
John Smith, you may stop now ..	0·33
,, ,, change to your other ear ..	0·33
,, ,, ,, ,, ,, ,, ,, .. (after pre-passage warning)	0·80 per subject

While the difference between the last class and either of the first two is not significant at the 5 per cent. level of confidence ($t_{data} = 1{\cdot}86$; $t_{0{\cdot}05} = 2{\cdot}13$) the difference does suggest that instructions might alter the set of a subject in such a way as to alter the chances of material in the rejected message being perceived. A further experiment was carried out on this point.

Experiment III

Two groups of 14 subjects were required to shadow one of two simultaneous dichotic messages. In some of the messages digits were interpolated towards the end of the message. These were sometimes present in both messages, sometimes only in one. The position of the numbers in the message and relative to each other in the two messages were varied, and controls with no numbers were also used, randomly inserted. One group of subjects was told that it would be asked questions about the content of the shadowed message at the end of each message: the other was specifically instructed to remember all the numbers that it could. The difference between the mean number of digits reported under the two conditions of set were analysed and submitted to a t test. In none of the cases, whether the score is the mean number of digits spoken during shadowing, nor in the number reported, nor the sum of these two, is the difference significant even at the 5 per cent. level of confidence. From this we may conclude that while it may be possible to alter set so as to increase the chances of material from the rejected message being perceived, it is at least rather difficult for neutral material: in the present case the numbers did not become "important" enough to break through the attentional barrier.

Discussion and Conclusions

The present results raise a problem that we may call the "identification paradox": that while apparently the verbal content of the rejected message is blocked below the level of conscious perception, nontheless a subject can respond to his own name.

Cherry's initial report on the use of dichotic shadowing described the sort of reports that subjects could give about the rejected message. They could distinguish between speech, noise and tones, could recognize clicks and gross changes in pitch such as the change from a man's to a woman's voice, etc. But the verbal content of the message was completely blocked, even to the extent of the subject being unable to say in what language the message was being spoken. Since the subjects could report as much as they did, peripheral blocking at the cochlea or cochlear nucleus such as has been artificially produced by Galambos (1955), and reported in the unanaesthetized cat by Hernádez-Péon, Scherrer, and Jouvet (1956), seems unlikely to be the full story in the present case. Such blocking could produce a failure to respond to the input to one ear, but is unlikely to cause a selective failure to respond such as is seen in the present case. It seems more likely that their mechanisms are concerned with gross switches from one modality to another such as Hernández-Péon was using in his experiments.

In the dichotic shadowing there seems to be only a selective block. It is generally held that while simple discriminations may be performed by the lower centres of the brain, the cortex is required for pattern discrimination of any complexity. Thus Sharpless and Jasper (1956) found that pitch discrimination and sound localization could be performed by a cat missing cortical areas Auditory I and II and with the medial geniculate body degenerated. But interval discrimination independent of pitch was impossible. So the present data suggest that the block in dichotic shadowing occurs at quite a high level, and that the block is central to some pattern analysing mechanism.

In this connection the work of Ingham (1957) reported in this *Journal* is relevant. He could show no evidence for a change in the absolute threshold for the perception of pure tones whether the attention was directed towards or away from the ear to which the stimulus was being presented. This result can be taken with Cherry's initial findings and those of the present writer and combined as follows. When attention is directed to one ear rather than the other there is no change in the threshold for the perception of simple stimuli, such as tones and meaningless noises. These are all treated similarly, and words are treated merely as sounds in this sense. This allows the subject to know that something has stimulated the ear whose message he rejects; it may be thought of as a general warning signal, that a sound has occurred to which the subject might need to respond. In addition to this there is a second system, concerned with pattern analysis and the extraction of verbal meaning from the stimulus input. At least some of this analysis is done below the level of conscious perception, and it is at the output side of this analyser that the block functions in dichotic listening. Certain patterns, those which are "important" to the subject, are selectively transmitted even when the block is in operation. An example of such an "important" stimulus is a person's name.

From this there follows the prediction that it should be possible to show a change in the threshold for the perception of meaning of words when the attention is directed away from the ear to which the message is presented. Such a change will not be found for simple auditory stimuli, nor for important stimuli such as a name. This prediction is being investigated.

We may conclude:

1. In a situation where a subject directs his attention to the reception of a message from one ear, and rejects a message from the other ear, almost none of the verbal content of the rejected message is able to penetrate the block set up.

2. A short list of simple words presented as the rejected message shows no trace of being remembered even when presented many times.

3. Subjectively "important" messages, such as a person's own name, can penetrate the block: thus a person will hear instructions if they are presented with his own name as part of the rejected message.

4. While perhaps not impossible, it is very difficult to make "neutral" material important enough to break through the block set up in dichotic shadowing.

The author wishes to thank Mr. R. Davis, who supervised the research, and the Medical Research Council, one of whose scholarships provided the financial assistance necessary for it to be carried out.

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BOOK REVIEWS

Elements of Psychology. By David Krech and Richard S. Crutchfield. New York. Alfred A. Knoff. 1958. Pp. xix + 700. 52s.

Introduction to Psychology. By Ernest R. Hilgard. London. Methuen & Co., Ltd. 1958. Pp. vii + 653. 55s.

Dr. Krech and Dr. Crutchfield have had the bold idea of trying to integrate biological and social psychology within the framework of an introductory text. Though hazardous, this task is clearly not impossible. Taking their departure from the study of perception, the authors proceed to a consideration of motives and emotions, and thence to the higher mental processes envisaged in terms of biological adaptation. A serious attempt follows to look at the individual in his adaptive relations with society. Although social aspects are thus well covered, the strength of the book undoubtedly lies in the care and thoroughness devoted to physiological issues. If for this reason alone, the book may be confidently recommended to medical students and others with biological training seeking an understandable account of modern psychology.

Dr. Hilgard has revised and largely rewritten his earlier text on human behaviour and its development. As one would expect from this author, the approach is cautious, level-headed and reassuringly experimental in tone. At the same time, Dr. Hilgard is by no means unaware of the claims of the less objective types of inquiry and of the contribution made by psycho-analysis to our understanding of human growth. Within the convention of current American psychology, this is a very useful account indeed.

Although admirable in their different ways, neither of these texts is wholly suitable for use in connexion with an Honours Degree course in psychology in a British University. As is well known, undergraduates in America tend to come to the subject younger than ours and with a very different educational background. Further, we lack the tradition of advanced academic instruction at post-graduate level. For these reasons, it is doubtful whether texts written primarily with a view to "adoption" by elementary psychology courses in the United States can ever really meet the needs of the British undergraduate. To say this is no disparagement of either book under review. The moral is surely that we must endeavour to meet our own needs by our own efforts.

O. L. Zangwill.

The Measurement of Colour. By W. D. Wright. 2nd Edition. London. Hilger and Watts, Ltd. 1958. Pp. ix + 263. Price 52s. net.

It is generally accepted that Professor Wright is the British Lion of colour vision, and it is interesting to consider how he has reached this remarkable position. He has not reached it by pursuing adventurous and exciting paths, or by offering to change our outlook on colour vision and colour blindness radically as a result of his experiments. On the contrary, his progress has been the result of activities tirelessly industrious but ideally reserved and cautious, so that one marvels that he could have dealt exhaustively with a subject fraught with so many traps, dangers and temptations as colour vision, without deviating so much as a hair's breadth from the most straight and narrow path. We might say that Wright has followed the perfect line of research laid down by Helmholtz, Abney, König and others, and has devoted his life to its exploration unswervingly.

This book, *The Measurement of Colour*, is one of Wright's great achievements. It is the second edition of a work published in 1944, and has been extensively revised, re-written and brought up to date. Unfortunately it was not possible to include in the book the final outcome of Stiles' and Burch's new determination of the colour matching functions with their magnificent elaboration of Wright's trichromatic colorimeter, at the National Physical Laboratory at Teddington. The aim and purpose of the book, of course, is to set forth in detail the principles, facts and applications of the C.I.E. system of colour measurement, and to compare it with others, such as the Ostwald and Munsell systems. This aim is approached by chapters on radiation and the visible spectrum, the receptive function of the eye and photometry and colorimetry. Interesting chapters follow, dealing with colorimeters and spectrophotometers, colour reproduction in printing, photography and colour television, and other practical applications of colour measurement to the problems of food colour, lighting, chemicals, paints, signal glasses, paper manufacture, meteorology,

astronomy and so on. There are eight plates, many illustrative diagrams and four appendices containing important tables. The value of the whole book for the practical colourist, as well as for theoretical and research purposes, can hardly be over-estimated.

To the psychologist, who is accustomed to a painstaking study of individual differences, it is disquieting to find experiments reported which have been carried out on one, two or small numbers of subjects, and that the essential data for the 1931 C.I.E. "standard observer," which still hold sway, were based on measurements by Guild on seven and by Wright on ten invididuals. Except for three diagrams, little or no evidence is given in this book about the variability of their colour vision among themselves or in relation to that of the population they were chosen to represent. It is reassuring that Stiles and Burch have dealt with a number more like fifty subjects in their recent work.

Fortunately, however, there are certain circumstances which restore confidence. The experiments and measurements involved in the determination of colour matching functions with Guild's, Wright's and Stiles's colorimeters are so exacting that they can be carried out satisfactorily only with expert or fairly practiced observers. To work on fifty observers would be a tremendous task, and the labour of really extensive observations would be too great to contemplate. Luckily, this exhaustive testing is probably not necessary. It is a remarkable fact in comparison with the great variability of human characteristics in general, such as height, weight, girth, intelligence and so on, that the colour vision of about 92 per cent. of the male population is so little variable that in fact even less than fifty subjects may be a very fair representative sample.

There may be other points in the book which give rise to disquiet, however. For instance, on page 47 it is said that we can be reasonably certain that the fovea itself is devoid of blue receptors, and on page 195 that hence in a television system there is little need to transmit information on the "E_B-E_Y channel" at the high frequencies which correspond to the fine detail of the picture. This is of practical importance, and probably quite sound, but it would be reassuring to have a presentation of satisfactory statistical data about the possible degrees and kinds of variation of blue sensitivity in the population for whom we are catering. It is therefore unfortunate that variations in colour sensitivity between individuals tend to be almost consistently ignored.

When the "standard observer" has been established, his measurements can be used in two directions. One is to treat colour as objective, and to estimate colours against his measurements as standards, which is explained in the later parts of the book in relation to industrial and other applications. The other is to use his measurements as norms and to estimate the deviations of the colour vision of the population against them. Some observations of the second kind are mentioned in the book. For practical purposes, however, a test of colour sensitivity has to be comparatively simple, to be applicable to anybody almost without prior training or practice, to confine itself to the directions in which experience has shown that the main forms of variability occur, and to be carried out under standardized conditions. These problems do not come within the scope of the book, which aims at treating colour on an objective basis independent of variations in individual sensitivity, although there is a short section on tests for colour blindness.

As a whole the work is well and clearly written, and, although it is full of technicalities, they are admirably presented, and those who wish to use its results and conclusions without detailed study of its theories, calculations and arguments, will find it readily understandable.

R. W. Pickford.

Personality and Temperament. By Solomon Diamond. New York. Harper & Brothers. 1957. Pp. 463 + vii. $6·00.

This book has several unusual features. The author, Professor of Psychology, at the Los Angeles State College, describes himself as "concerned about achieving a rapprochement of clinical and experimental viewpoints" and he is far more successful in this aim than are most writers on personality and temperament. Professor Diamond's attack has the virtues of eclecticism but surprisingly few of its defects.

He makes use of most of the approaches that have been employed by psychologists—developmental, comparative, factorial, typological, behaviouristic, physiological, psychometric and psycho-analytic—and his selection is well made and his exposition clear. Professor Diamond deals sympathetically with the various viewpoints but he is, on occasion, a shrewd critic. In the chapter on Levels of Maturity, for instance, when discussing the Vineland Scale of "social maturity" he makes the point that "there is . . . an implicit assumption that as examples of mature behaviour make their appearance, the tendencies to immature behaviour are being progressively displaced. This reciprocal

relationship is not necessary." Again, after an enlightening account of Eysenck's work on the "neuroticism factor" by means of "criterion analysis," Diamond comments: "Its results deserve serious attention, but they leave us to a degree uncertain whether the investigator has penetrated the mystery of nature, or forced it to his will."

Professor Diamond is gifted, apparently, with a generous supply of robust common sense, which has withstood the impact of intensive reading and thinking in the field of psychology and more especially in that wide, meandering strip known as "personality." In his chapter on Interests, for example, he says: "As always when the question of interacting hereditary and environmental factors arises, it is more profitable to be alert to the manner of their interaction than to pit them against each other like gladiators, with the partisan hope that one of these forces may annihilate the other. . . . In fact they have been living peacefully together for a long time, except in our minds." And when writing on the Defence of the Self-concept, he puts his finger on a fallacy of the type which constantly occurs in personality theory. ". . . we are led to suspect that all our self-aggrandizing feelings arise as compensations for our initial feeling of smallness—that there can be no 'feeling of superiority' except as the expression of a deeper-lying feeling of inferiority. Although there are certainly a great many cases in which this Adlerian theory is applicable, we should not deny the possibility that feelings of self-satisfaction may grow directly out of our positive achievements."

Commonsensical again, yet insightful, is Professor Diamond's emphasis on "the general principle that mental abnormalities represent extreme developments of normal processes" and on the "motivational and non-cognitive basis" of "individuality in the intellectual sphere." On the other hand, he is sometimes guilty of naïveté. He tells us, for instance, that on a self-rating scale "only about 1 per cent. [of a group of high-school pupils] put themselves in the bottom fifth of the class with regard to honesty"! We are later informed, with equal gravity, that Strong's masculinity-feminity scale "is also available for the women's form."

Such are minor points, however, in a generally satisfactory book. *Personality and Temperament* is a useful reference book in that it deals comprehensively and fairly with much of the relevant literature and has, itself, an excellent list of references. The book can also be read, instructively and enjoyably, from cover to cover.

A. W. Heim.

PROCEEDINGS OF THE EXPERIMENTAL PSYCHOLOGY GROUP, 1958

25–27th March, 1958. Extended Meeting at Cambridge. *1st Session:* "The processing of positive and negative information," by P. Wason (by invitation). *2nd Session:* "Perceptual skills in blind children," by B. R. Gomulicki (by invitation). "Reasoning about proportional relationships," by I. M. L. Hunter. *3rd Session:* "Optimal loading in an inspection task," by E. C. Poulton. "A basis for judgments of sensory intensity," by R. M. Warren (by invitation). *4th Session:* Start of Joint Meeting with the Association for the Study of Animal Behaviour. "Stereoscopic vision: binocular temporal summation," by R. Efron. "Tactile localization," by G. H. Wright and C. R. E. Halnan. "Some effects of reserpine on behaviour in the rat," by P. H. Glow. "Taste discimination in goats," by F. R. Bell. *5th Session:* "The analysis of optomotoric reactions in insects and crustacea," by B. Hassenstein. "A test of a theory of shape discrimination in octopus," by N. S. Sutherland. "Tactile discrimination in octopus," by M. S. Wells. *6th Session:* "Social interactions in discrimination learning," by P. Klopfer. "Some problems in localization of hormonal action on the C.N.S." by R. Warren.

7th–9th July, 1958. Extended Meeting at Oxford. *1st Session:* "Attending and responding in refractory delays," by R. Davis. "Binocular interactions in subject effects produced by viewing intermittent stimuli," by I. P. Howard (by invitation). "The effect of warnings on thresholds," by M. Treisman (by invitation). "Effects of

brainstem lesions in cats on perception of pain," by R. Melzack. *2nd Session:* "Interaction of generalization in two dimensions," by D. G. Ellson (by invitation). *3rd Session:* "The rôle of speech in perceptual-motor discimination in imbeciles," by B. Hermelin and N. O'Connor (by invitation). "The mechanisms of visual tracking," by C. Rashbass (by invitation). "The discrimination of after-images," by G. S. Brindley (by invitation). *4th Session:* "Reasoning and extinction in the rat," by J. A. Deutsch and J. K. Clarkson. "Retinal sensitivity and interaction in the visual system," by M. H. Pirenne (by invitation). *5th Session:* "Dark adaptation: the relation between threshold and the concentration of visual pigment,' by W. H. Rushton (by invitation). "The stability of the visual field during eye movements and the perception of real and apparent movement," by R. L. Gregory.

29th–31st December, 1958. 11th Annual General Meeting at Birkbeck College, London. *1st Session:* "The effect of a constant environment on feeding, drinking and body weight in rats," by K. Weiss and H. M. B. Hurwitz. "Alterations in size constancy associated with cerebral lesions in man," by M. Wyke (by invitation). "Rhythm, anxiety, and human 'internal inhibition'," by I. Oswald (by invitation). "The effect of stress on food demand and body weight," by H. M. B. Hurwitz. *2nd Session:* "The perception of causation," by R. Eifermann (by invitation). "The problem of scale in the geometry of binocular vision," by A. Zajaczkowska (by invitation). *3rd Session:* "Controlling the focus of the human eye," by J. Robson (by invitation). "Simple apparatus for monitoring eye position" (demonstration) by B. Shackel. "A 'solid image' microscope" (demonstration) by R. L. Gregory and P. E. Donaldson. *4th and 5th Sessions:* "The selective effect of set on tachistoscopic perception," by J. Brown. Symposium on "The Psychological Refractory Period,": "Some exceptions to intermittent behaviour," by A. Elithorn. "Is the psychological refractory period due to limited channel capacity?" by A. M. Halliday (by invitation). "Evidence of a single-channel decision mechanism limiting performance in a serial reaction-time task," by A. T. Welford.

Committee, 1959. President: Professor G. C. Drew.

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